

Observations

ON THE
HISTORY AND CAUSE
OF
ASTHMA;

AND
A Review of
"A Practical Enquiry on Disordered Respiration;"

IN A LETTER TO
ROBERT BREE, M.D.
The Author of that Work.

BY GEORGE LIPSCOMB,

SURGEON, AT BIRMINGHAM.

Sc. Sc.



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GOVERNMENT

OF THE

UNITED STATES

ARMY

REGIMENTAL

AND BATTAL

COMMISSIONS

AND RECORDS

OF THE

UNITED STATES

ARMY

AND NAVY

DEPARTMENT OF THE ARMY

To the Public.

A VARIETY of malicious rumours, industriously circulated, and encouraged from motives of envy, or of personal enmity, having unfortunately so far succeeded in prejudicing the minds of the public, as to occasion the Author an unusual degree of leisure; he thought that he could not employ himself in a manner more conducive to the benefit of his fellow-creatures, or more becoming his *professional* reputation, than by endeavouring to elucidate the history of a very prevalent and distressing disease, which has been hitherto but ill explained, and very unsuccessfully treated.

At the same time he embraces the opportunity, which the present publication affords him, of assuring those liberal friends, who still honour him with their confidence and patronage, that neither time nor accident can diminish the grateful sense which he

entertains of their kindness; and while he deeply regrets the insurmountable obstacles which have prevented him from publicly refuting calumnies, which, in the multifarious shapes they have assumed, have *always* been so guarded by artifice, or modelled by intrigue, as to elude any *direct blow*, even if his fortune had afforded him the means of seeking that redress to which the justice of his cause entitles him:—he rests, with confidence and assurance, on the continuance of their protection, whose friendship for him has either been founded on habits of intimacy and long established acquaintance, or has generously arisen out of a regard for justice, and an hatred of oppression.

He earnestly hopes that Time (the universal Physician) the rectitude of his conduct, and his professional industry, will gradually obliterate any unfavourable impressions which may have been made on those, who, being strangers to him, have had no opportunity of investigating the causes of the persecutions

persecutions which he has undeservedly suffered.

The loss of the pretended attachment of some who, actuated by motives of interest or of policy, were once solicitous of being reckoned in the number of his friends; and who, whenever a *proof* of their sincerity shall be demanded, will desert their new associates with as much pusillanimity as they have deserted him; and with as much alacrity as they have fled from the performance of solemn promises and reiterated asseverations; is not to be considered as a real misfortune: and such he *condescends* to mention, only that he may remind them, as he has already observed in another publication* that "*popular applause is lighter than a feather or a bubble, and less substantial than a dream;*" and that although the friendship of the GOOD and the WISE will ever be the object of his

* A Journey into Cornwall, &c. interspersed with remarks, moral, historical, literary and political, 8vo. Rivingtons, St. Paul's Church Yard, London, 1799.

highest ambition; yet he trusts he shall constantly support that manly independence of spirit, which will enable him to sustain, without a single emotion of regret, the loss of any intimacy with those, who, however superior to him by birth or station, have no claim to the epithet of *wise* or *virtuous*.

And if there yet remain some malignant spirits who experience a delight in the invention or propagation of calumny and detraction: if the itching ears of envy and credulity still feel a gratification in listening to the clamours of scandal, or the whisperings of falsehood:—to these he desires to say, that although they may *rob* him of “a *good* report,” they cannot deprive him of a *good* conscience: nor shall all the efforts of envy, of malignity, or of persecution, make him relinquish the pleasing hope, of still contributing to *advance* and to *improve* the practice of the profession, *in* which he is engaged, *for* which he was educated, and *to* which he is in some measure naturalized, by
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its having devolved on him, through a respectable line of ancestry.

The Author thinks it would be impertinent to trouble the public, with any explanation of the circumstances which have induced him to select Dr. BREE's "Practical Enquiry on Disordered Respiration," for the particular subject of his animadversions: but as a literary controversy, on a professional topic, between a Surgeon and a Physician *resident in the same Town*, will, perhaps, be supposed to have arisen from personal pique, envy, or jealousy; it may be proper for him to obviate the possibility of any such misapprehension, by informing his readers, that Dr. BREE is equally unknown to him as a gentleman and a physician.

Birmingham, March 10, 1800.

ERRATA.

Page 20, line 10, for ? insert .

Note, line ult. for humanâ read humanâ.

27, line 8, and 19, and in other places, for distention read distension

34, Note, line 7, for "Willis," read Willis

52, Note, line 7 for allisque, read alisque

55, Note, line ult. after sensation, insert ?

68, line 16, for with, read and

79, Note, line 6, after nerves, insert ;

81, Note, line ult. for Expements, read Experiments.

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OBSERVATIONS
ON THE
HISTORY AND CAUSE
OF
ASTHMA,
&c. &c.

S I R,

THE perusal of your “Practical Enquiry on Disordered Respiration,” having induced me to consider the Nature and Cause of Asthma with some attention, I take the liberty of addressing to you a few observations on this subject.

I do not think it necessary to preface my remarks with an apology for their freedom, because every Author who introduces to the world either an history of new facts, or an illustration of principles already established, is amenable to society for the consequences of his opinions, and obnoxious to the criticism of those who doubt the truth of his reasoning, or dissent from his conclusions.

Although your “Enquiry” had made its appearance more than two years before it happened

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to fall into my hands; neither the length of time which has elapsed since its publication, nor the difficulty of combating some opinions which appear to me objectionable, without exposing myself to the imputation of satirical malevolence or captious irascibility, shall therefore deter me from what I conceive to be a serious duty to the public:— from stating, without reserve or equivocation, how far the doctrines contained in your “Practical Enquiry” have a tendency to produce any useful improvement in the method of treating Convulsive Asthma; and how far your reasonings on that Disease have elucidated the history of its nature and cause.

I read your publication, Sir, with that degree of attention which the discussion of a subject so highly important seemed to require, and will confess that, when I reflected on the want of precision among Authors, which you have been pleased to adduce in your preface, in vindication of a new arrangement of the causes of Asthma; when I reflected on the important assistance which has been derived to the study of Medicine from innumerable recent improvements and discoveries in the sister sciences; it afforded me pleasure to find, that a gentleman of learning and industry had chosen this Disease for the particular object of his studies and investigation: and, when I understood that the author's clinical observations had been aided by a “personal acquaintance with the disease,” I looked forward
with

with eagerness and confidence, for the attainment of some useful additions to the general stock of medical knowledge.

It is not, however, to complain of my disappointment, that I take the liberty of addressing you; but that I may, with all fairness and all candour, investigate the degree of credit due to your theory, and the degree of attention due to those principles of reasoning, on which you have grounded the practice you recommend.

They who read, only to know the opinions of others, forget one of the most obvious and important uses of human intellect;—they forget that this invaluable gift was bestowed upon them, that they might be enabled to weigh every sentiment, and to appreciate every remark:—they should study as well as pronounce, should be cautious in the admission of new theories, and examine the validity of every argument by the rules of just criticism; should yield nothing to the emptiness of founding titles, or the dogmas of pompous erudition; but minutely regarding those substantial and indefeasible arguments, which are only founded on observation and experience, should reject every innovation, and discountenance, every affected improvement, which will not stand the test of actual examination.

Every Author has an undoubted right to make use of what terms he pleases; but whenever he

introduces an epithet not generally understood, or not usually applied in the same signification as that in which he uses it, it is incumbent upon him to define the specific meaning which he intends to convey.

Phyfic, in itself an abstruse science, has become almost unintelligible, from the immense quantity of ill-defined terms which have been introduced, by those who pretended to illustrate the study of it. To understand the language of ancient writers, replete with ambiguity, and stuffed with a farrago of quaint and indeterminate epithets, is now a task so arduous, that few students have sufficient patience, to combat this formidable obstacle to the improvement of Science:—but although this circumstance, by compelling men, who possess more of genius than of industry, *to think for themselves*, has been in a certain degree, instrumental in the production of those valuable improvements in the different branches of Medicine, which have distinguished the present age; it is to be lamented, that the custom of embarrassing the reader with technical terms, never well defined, and consequently seldom perfectly understood, has been extended even to our own times, and, I am sorry to say, has reached your “Practical Enquiry *on* Disordered Respiration.”

But I do not forget that I am to combat your *opinions*, rather than the *words* in which they are conveyed: that I am not to attempt a review of
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the whole of your work, to marshal its inaccuracies, or to expose the peculiar ambiguity of, what a little fastidiousness might denominate, its inflated periods. I hasten, therefore, to fulfil the task which I have undertaken, without further preface or introduction.

Your first section is devoted to an anatomical sketch of the Lungs, and other organs of Respiration; and, a mere general view of these parts being all which is required, to elucidate the history of Asthma, I shall not cavil at the want of a more accurate description; nor, as I know the difficulty of adapting specific terms, capable of illustrating many of the appearances which are observable in the animal machine, am I captious about the "blood-vessels spread*" and interwoven like the "meshes of a net," or "the perfect† net-work" formed by the lymphatics: although neither of them may be very correctly explained by these allusions. But, I must confess, that the inference which you have deduced from the slender size of the nerves which supply the Lungs, appears scarcely reconcilable to general observation.

You remark, that "the ‡pulmonary nerves "being so small, the lungs have little sensation." Does the acuteness of sensation, then, depend on

* Dr. Bree's Practical Enquiry ON Disordered Respiration, p. 4.

† Ibid.

‡ Ibid. and 171.

the size of the nerve exposed to the influence of stimuli? Certainly not: for a much more perfect and correct idea is produced in the mind, from the infinitely minute terminations of the nerves of the skin, than arises from the application of any stimulus to a large surface, or a large proportion of the greater nerves.

Let us attend a little to experience. The fimbriated extremities of the olfactory nerves, distributed to the membrane which lines the nostrils, are surely not deficient in sensibility; for they may be stimulated by a portion of matter so small, that we have no means of affixing to it any determinate idea of quantity: although their origin is not so large but that they are frequently broken in dissections, by raising up the brain, in order to demonstrate their exit from it. And to return to the lungs;—it will not be denied, that the pain felt in peripneumonic inflammation is at least equal in degree, to the pain which accompanies inflammation of any other membrane of the body.

Whence happens it, Sir, that if the lungs have “little sensation” they should be so excessively liable to be irritated by every atom of extraneous matter received into them? Are the nerves you *allow* them, distributed only to the “**intermediate*” “intervals” which you speak of, between the lobules? Or, how is your doctrine of the insensibility of this

* Practical Enquiry, p. 2.

viscus, reconcilable to the irritability of the delicate membrane which lines the Trachea and its branches, even in their minutest ramifications?

I perfectly agree with you, that “the lungs* “have no inherent power by which they can attract “air;” and will acknowledge that when I first read the remark, I was a little puzzled to know who had suspected the contrary: till I recollected that *Ambrose Parey*, more than two hundred years ago, among other wise suggestions, had started an idea, that the lungs had “*a disposition to imbibe* pus, which in “pleurifies and other purulent abscesses,” said he, “is poured † into the cavity of the thorax.”

I come in the next place to consider an observation more singular than, and I apprehend equally indefensible with that respecting the insensibility of the lungs.

“Phthisis ‡ and Asthma,” you remark, “comprehend the features of every serious indisposition “which can generally attack the lungs, and they illustrate mutually the character of each. It will be “found, that in their regular, simple, and uncomplicated forms, the remedies of one are the exciting “causes of the other, and the causes of the one are

* Practical Enquiry p. 5.

† Amb. Parey, Opera, lib. iv.

‡ Practical Enquiry, p. 11.

“recipro-

“reciprocally the remedies of the other.” This paradoxical assertion I cannot think of holding up to the admiration of my readers, with any other view, than that it may possibly contribute to the improvement of Science ; or, to use your own words, *to the gain of humanity*, by a display of error.

In refutation of the former part of the sentence, I shall only beg leave to remind you, that the disease called Hydro-thorax, appears justly entitled, by its oppressive symptoms, and fatal effect, to be ranked with Asthma and Phthisis, as, at least, “*a serious indisposition*” affecting the lungs.

If the position which you have advanced in the latter, be admitted, it will indeed afford a most cogent reason for never attempting the cure of Asthma ; since you have discovered, that such an apparent benefit would necessarily be succeeded by a disease to which, you acknowledge, that “*medical* treatment is opposed, without hope, and without success :*” which by the way, seems to render it extremely problematical, whether you are acquainted with the exciting causes of Asthma ; for, if the “*remedies of Phthisis*” (according to your preceding remark) be the exciting causes of Asthma, and if Phthisis be an incurable disease ; *What are its remedies ?*

* Practical Enquiry, p. 11.

Again ;

Again;—in another place you have said, that Asthma is “excited* by irritation,” and you *allow* it to proceed from “various remote causes.”—May I ask then, which of the remedies employed for the removal of the *irritation*, can you possibly dread, as the exciting cause of Phthisis? and which of those remote causes of the irritation, you have alluded to, do you conceive likely to remedy Phthisis? Vinegar is one of the remedies for the paroxysm, on the use of which, you have laid a considerable stress: I have, therefore, selected it as the ground of another question.—Do you consider it capable of exciting Phthisis? One more question, and I have done. You have particularized, “severe study” as one of the remote causes of a predisposition to Asthma.—Do you think it would at all conduce to the cure of Phthisis?

On the merits of your definition of Asthma, I would wish to say a few words.

It is avowedly taken from those of *Willis* and *Hoffman*, and partaking of the nature of both, is liable to the objections which have been made to each of them. For myself, I hold all nosological disquisitions as of but small importance, in the history, and consequently in the study of diseases. A definition can only be a good one, when it conveys a clear

* Practical Enquiry, p. 11.

and correct idea of a disease, and exhibits the outline of its character, in such a light, as to distinguish it from every other disease.

In order to define a disease accurately, it must be perfectly understood: but a disease may be perfectly understood, so far as the limited powers of the human understanding can be said to attain *perfect* knowledge, and yet it may be almost impossible to give an exact definition of it.

Much labour and study being required for that purpose, it is to be feared, that while Authors have been racking their brains for comprehensive terms, whereby they might define diseases, a great portion of time and of industry has been wasted, which might have been more advantageously employed in the investigation of the symptoms and progress of those diseases, and in the establishment of opinions founded on clinical experience, which is the only true basis of practical knowledge.

You have been pleased to quote some authorities, on the quantity* of atmospheric air received at each inspiration; and have noticed the curious phenomenon of divers in the pearl fisheries remaining under water, for a considerable length of time. I take leave to introduce a circumstance which you seem to have overlooked, namely, that in persons accustomed to diving from an early period of life, the *foramen ovale* remains open. The freedom of the circu-

* Practical Enquiry, p. 20, 21.

lation, therefore, in these amphibii, is not so much interrupted, as would otherwise happen: and as water is known to contain a much larger proportion of *oxygen*, than is mixed with atmospheric air, may it not be suspected, that a certain quantity of this necessary fluid is absorbed into the system during submerfion?

I do not remember by whom the fact I have mentioned, was originally discovered; but it was well known in the last century, and frequently demonstrated by *M. Dionis*, a famous French Surgeon and Anatomist.

In Newfoundland-dogs also, the *foramen ovale* is found open. I have, myself, once seen it, in the adult subject, and from the circumstance of the individual being a native of the coast of Africa, and a failor, I was induced to suppose that he also might have been a diver; but respecting this, I had no opportunity of obtaining authentic information.

You remark, that some "animals* pass whole "winters in sleep, and emerge from it in spring "with *increase of fat*." This does not, however, accord with general observation: for it is well known, that Bats are frequently discovered in their torpid state, but that they are invariably found *lean*, in a degree proportionate to the length of time

* Practical Enquiry, p. 21, 22.

which has elapsed, from the commencement of their protracted night. The Marmot also, another of the animals whose existence is obscured by torpid inactivity, is expressly described, as having acquired a degree of plumpness at the approach of winter, but at the time of its restoration, (in the month of April) as being constantly* reduced to a state of great emaciation: and even in appearance, almost a skeleton.

The quantity of oxygen required for the maintenance of the vital principle, varies greatly, in different animals; and perhaps, in the same proportion as the degrees of temperature of their respective bodies.

It is obvious, that a much smaller quantity of air is received into the lungs during sleep; and, that whenever a person has slept in the same clothing commonly worn when awake, the sensation of cold is always perceived, as soon as the sleep terminates. Those animals which sleep for several months together, have their natural heat considerably diminished.

The Toad, which affords, perhaps, the most singular example, in the whole series of animated nature, of long abstinence from pure air; or rather, of life being kept up, by a very small quantity of oxygen, may likewise be adduced in proof of what

* Martyn's Dict. of Nat. History. Art. Marmot.

has been advanced. I have, however, sometimes entertained an idea, that the last named animal may possibly be furnished with the means of throwing out from its body, *gas*, in a state capable of re-combination with other substances, in the very confined atmosphere around it, (as, for example, in the cavity of a block of marble, little larger than its body) so as to form a quantity of oxygen sufficient for its nourishment.

I am not informed whether this animal becomes emaciated or not, in consequence of its confinement, above alluded to: but as all the rest of this class of sleepers are reduced in size, so it is probable that a century or two, may produce *some little effect* on the toad likewise.

I pass over, in total silence, the numerous descriptions of morbid appearances on dissection, which with so great industry, and even labour, you have collected from ancient writers; because, in some cases, it is confessed, and, in others, it is highly probable, that *no asthmatic affection* prevailed.

It is an incontrovertible fact, that the lungs may be gradually compressed, to almost half their usual size, so that the inferior parts of the lobes shall remain impervious to the air, inhaled by the larger branches of the trachea; and no Asthma be produced: of which I have myself been an eye witness, in the case of *William Hunt*, of the parish of *St. Nicholas*, in *Warwick*, who died of hydrothorax;

thorax ; the particulars of which may possibly, hereafter, be presented to the public.

I shall not trouble you with any remarks on your divisions of "convulsive Asthma," in the second part of your "Practical Enquiry;" nor on the "*Asthma Exanthematicum*,"* proceeding expressly "from the retrocession of an exanthematous acrimony," but leave it, to speak for itself, *in its own words*.

But, I cannot omit the passage, in which you mention the increase of energy, as sometimes proceeding to a degree, which in itself constitutes a disease. You adduce, by way of example, the effect of "acid Bile" in the following words. "Acrid Bile† in the intestines occasions extraordinary peristaltic motion, and if that irritating material is not discharged from the bowels, by the ordinary exercise of their powers," (I presume you mean *such extraordinary* peristaltic motion) "an inverted motion will arise," &c.

A great deal has been said about this *inverted motion*, and even of the intestinal canal being tied in knots. Whoever considers, for a moment, the form and connexion of these organs, will not, however, require any argument to convince him, of the impossibility of the latter effect being pro-

* Practical Enquiry, p. 75.

† Ibid. 161.

duced,

duced, unless the intestines could be previously separated from their connecting bands, the mesentery and mesocolon. As well might the ruffle of a shirt be tied in knots, while it maintains its attachment to the wrist-band.

In short, it is a fallacious statement, deduced from the sensations of the patient; and entirely unsupported by any real evidence whatever.

It is very true, that Introsusceptio does take place; and probably, in consequence of extraordinary excitement of the peristaltic motion; but even this is far from proving that the motion has been *inverted*. It rather proves the contrary, at least in several instances, which I have had an opportunity of examining, and where the recipient portion of the tube was invariably found to be the inferior continuation of that part which it enveloped. Now, it is well known, that no *probability* of inverted motion can be urged, from any difference of size, in the cavity of the jejunum or ileum, and that the tube of the colon, which is most frequently, the seat of the disease, is nearly of an equal diameter throughout its whole length. There is, in fact, but one proof which can be adduced of the occurrence of inverted action, to wit, the rejection of *scybalæ* through the stomach. This is so rare an occurrence, that a practitioner may enjoy very extensive opportunities of observation, without meeting with it once, in his whole life. Being therefore so
uncommon

uncommon, it ought not to be advanced as a foundation for general reasoning: besides, it should be remembered, that even in some of the few cases of the disease which have been authenticated, the occurrence of this symptom was induced by a repetition of violently active emetics; which may be allowed capable of effecting an alteration in the system, which the uninterrupted progress of a disease would never have produced. If, however, the peristaltic motion were reversed in cases of intussusception, we should undoubtedly often meet with the inferior portion of the intestine intruded upwards.

And, as very great inflammation, in all cases; and, even in most of them, a mortification precedes death; it frequently happens, that there is great difficulty in determining the precise spot at which the disease commenced: and the experience which I have had of this difficulty, has led me to suspect, that the accounts delivered to us, of the appearances, on dissection, have been sometimes not very correct.

It is much to be regretted that the opportunities of anatomical researches are so often denied, to the commendable solicitations of the faculty:—were these advantages more general, every man of industry and talents, would be enabled to form an infinitely more correct idea of the nature of those diseases, which are connected with evident disorganization,

nization, than he can ever obtain, by the most attentive perusal of Bonetus or Morgagni.

The ancients, who undertook their dissections frequently with a view to the illustration of false, and now, justly exploded theory, were extremely apt to have their minds grossly biased by some favourite hypothesis; and we, who enjoy the clearer light of modern discoveries and improvements, may readily trace, in their writings, the unsuccessful efforts which were employed, to prop up the visionary superstructure, founded on their imagination, rather than on their judgment. Of this, I shall have occasion hereafter to speak more particularly.

The inflammation produced in some cases of intussusception, as I have before observed, is known to proceed even to the death of the part: and it frequently happens, that a perfect adhesion of the folds takes place, and that a mass of granulations is formed, of a large size, with which the folds of the intestine are so consolidated, that the precise origin of the disease cannot possibly be traced.

An instance of this kind occurred in my own practice, and was demonstrated on dissection, in the body of *Thomas Blackford*, of *Barford*, near *Warwick*. The disease in this particular case had been undoubtedly occasioned by the injudicious and excessive use of cathartic medicines. A very large portion of the colon was protruded through the sigmoid flex-

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ure, where its duplicatures were united, and consolidated in a vast mass of granulations, which for some time previous to his death, formed a considerable tumour externally ; and ultimately obliterated, entirely, the cavity of the intestine.

From a consideration of these circumstances, I can easily conceive that where the origin of the disease has been rendered obscure, and the mind of the examiner predisposed to the belief of inverted action, some cases may have been adduced in support of a favourite theory, which, a more unbiaſſed examination would have had a direct tendency to contradict.

You appear to be anxious, Sir, for the establishment of an implicit dependence on the “accuracy of observation,” as well as the “fidelity” of ancient authors, whose works you are pleased to call “classical.”

I request your attention to a few remarks, which have been suggested by observing the deference you every where pay to the sages of antiquity.

The early writers on physic, have filled their works with a great deal of nonsense about *black bile*, and its deleterious* consequences. Has any single

* Hippocrates maintained this opinion with so much vehemence, that, as his learned commentator has observed, “Hippocratis sententia
“ fuit OMNES putridas febres a bile nasci.”

dissection,

dissection, from the creation of the world to the present day, tended to countenance even the shadow of such a theory? and yet, do we find our ancestors the more inclined to part with their favourite opinion?

This obstinate adherence to erroneous doctrines is not, perhaps, to be urged in objection to the weight of their authority, as clinical historians; but, as that obstinacy resulted, in a great measure, from the inaccurate knowledge which they had, of the structure and natural history of the body; I am myself so much a modern, that I will sometimes venture to doubt the correctness of their statements respecting the appearances after death. Nor, do I conceive, that I am unworthily detracting from the merit of the ancients, when I endeavour to place them in the situation, to which their industry and exertions have really entitled them; although in a niche somewhat lower than that to which they have been undeservedly raised, by the devotion of their superstitious admirers.

What is it? but to offer the respect of rational and deliberate opinion, instead of blind and inconsiderate adulation. I regard the writings of the ancients, as objects of criticism, rather than of delight; and although I endeavour to study them without prejudice, I am cautious not to suffer the garb* which envelopes them, to awe me into veneration.

* The eye of philosophy should penetrate the covering which time has woven to conceal the errors of ancient writers; and, I hope,

It is impossible to know the true value of the talisman, till the charm which guards it be dissolved. The real form of the saint cannot be understood, till the shrine which encloses him be examined: and shall he who respectfully approaches, to view with the eye of reason the long concealed treasure, be accused of sacrilege? as if with unhallowed hands, he had presumed to violate the sacred inclosure, or with profane impiety, to scatter the holy relics?

In the darker ages of physic, and before the knowledge† of anatomy was either generally, or in-

we may freely investigate the credit due to their authority, without violating THE SPIRIT of the well known axiom, "de mortuis, nil nisi bonum:" to which I most readily yield my assent, "for* though (after the approbation of the good and wise) one cannot wish any thing better than to be heartily abused by the envious and malignant in this life, because it is as certain a sign of one's merit, as a dog's barking at the moon is of her brightness; yet the veil which death draws over us, is so sacred, that the throwing dirt there, has been esteemed at all times, and by all people, a profanation." While we admit the truth of this remark, however, let me observe, that a precept of far greater importance in point of morality, is too often and too commonly disregarded—"de viventibus, nil nisi verum."

* Warburton.

† Among a thousand other absurdities, it has been asserted, that the bile of Negroes is black instead of yellow; and that the colour of their skin is occasioned by a deposit of this fluid in the cellular membrane. See Doctor Barrere's "Dissertation on the physical cause of the colour of Negroes." Could this LEARNED physician have ever seen the dissection of an African?

Again, in one of the books which has been attributed to Hippocrates himself;—I mean the treatise "de naturâ humanâ;" it is said that

deed almost at all understood, the opinions of those who wrote on diseases can be but of little importance. Their theory was, at best, only ingenious conjecture: their practise only uncertain empiricism. The dawn of a more resplendent æra, at length appeared; and perhaps it had been well for mankind, if the sun which then arose, had burnt up the ancient speculations of theorists altogether.

Unhappily, however, even among the moderns, we still find phlegmatic writers, who continue to look up to these "*blind guides*" with all the superstitious ardour of antiquity, and still *venerate* the dignity of Greek and Arabian physicians, when their patient industry in recording the symptoms of diseases has only entitled them to our esteem.

Hippocrates, whom you, Sir, as well as some other modern authors, have again and again dignified, with the preeminent title of the "FATHER of Medicine" (as if he had created it) may lose no inconsiderable share of his honours, and *all* his divinity, without any degree of biographical injustice. *Galen*, *Sennertus*, and *Riverius**, scarcely de-

that four pairs of veins arise from the head, and are distributed through the body! And although Galen might possibly have been influenced by other motives than the love of truth, when he ascribed that work to Hippocrates, it cannot be denied, that both these authors, whenever they have attempted to reason on the principles of Anatomy and Physiology, have perpetually fallen into absurdities, quite as ridiculous as that hypothesis.

* "Attamen in magno per me servantur honore;

"Pulveris et cariem plumatis tergo flabellis."

serve to be mentioned, unless in order to prove, that the most splendid talents, the most industrious perseverance, and the liveliest imagination, unaided by the benefits of dissections accurately performed, and a clear* and comprehensive view of the natural history of the human body, are insufficient to constitute any thing like a knowledge of the nature and cure of diseases. It is not, however, my intention to dissuade the student from *perusing* the works of the ancients, for it is impossible that a man should know what *can be done* in the profession of physic, without being first informed, of what *has been done* already; and without such previous knowledge, he cannot tell whether he is the author of any improvement or not. I only mean to warn him against that *implicit confidence* in the early writers, which tends to the permanent establishment of error, and to the total suppression of ingenuity.

* The ancients supposed that BILE and PHLEGM were two of the causes of inflammation of the lungs, and that its degree of danger depended on the TEMPERAMENT (as they called it) of a patient, being more or less analagous to the particular nature of those causes! supporting their opinions by arguments worthy of such absurdities.

Thus, the doctrine maintained by Galen was “ Peripneumoniam
 “ pituitosam minus periculi habere quam biliosam. Pulmonem au-
 “ tem temperies est calida, minimeque frigida. Quare pituitosa pul-
 “ monis phlegmone parti, non erit analoga; ergo et periculosior.
 “ Pulmones sunt molles, non duri: sunt rari, non densi: sunt laxi:
 “ si habitum inspexerimus, facile animadvertemus biliosam inflam-
 “ mationem minime esse analogam substantiæ pulmonum: attamen
 “ temperiei pulmonum analoga est hæc inflammatio biliosa. Quare
 “ AVICENNA ex GALENO BENE dixit, erysipelatodem phlegmo-
 “ nem pulmonis periculosiorem esse pituitosam.”

I have

I have now arrived at your observations on the theory of spasm, maintained by the late Professor Cullen.

After quoting Dr. Stark's description of the appearance of the lungs on dissection; you add, "Dissection* then, has not elucidated the mystery of spasmodic constriction of the bronchia, but it will be replied, that the spasm is relaxed before death, and thus eludes the examination of anatomy. It is, however, remarkable that this state of constriction has not appeared in some instances after death, when an attendant disease of more rapid and *fatal progress* than the Asthma, may have *finished* a patient under the dominion of the paroxysm of the latter."

With all due deference, Sir, if one of the established laws of animal death, be a relaxation of the muscles of the body (and I call upon you to correct me, if it be *not*, by the history of a single case, well authenticated, in which it has ever been departed from); it is evidently of no consequence at all, whether the spasmodic constriction has been occasioned by Asthma or by any other disease; nor to what particular cause the extinction of the vital spark be referred: for it will be *equally impossible* to *demonstrate* the existence of spasm on dissection.

What muscles are found rigid in death? None.
What fibres are discovered in a state of contraction?

* Practical Enquiry, p. 175.

None.

None*. Spasm, therefore, cannot be *demonstrated* by any other dissection but by the method which furnished the Greeks with all the little knowledge they had, of the structure of the body, namely, by the unparalleled barbarity of dissecting living men: a practise which the learned physicians among that *humane* and *enlightened* people, considered as much less painful and offensive to the *manes*, than the dissection of dead bodies.

But the want of such demonstration is no more a proof that spasmodic constriction had not preceded the death of a patient, than the loss of contractility in the skin, is a proof of its never having been furnished with the power of contracting during life;—a quality which I am persuaded no man will venture to deny.

Hence, I contend that the inference which you have drawn, that “as† no appearance of spasm

* “There is a power constantly contracting the muscular fibres of a LIVING animal, to a shorter length than they would be contracted by their elasticity.”

Dr. George Fordyce’s “Second Dissertation on Fever,” page 75.

I shall have occasion, frequently, to refer to this ingenious author, to whom I am indebted for many of the principles on which I have founded the arguments contained in these pages. In some instances, I have accidentally caught the Doctor’s mode of expression; in a few others I have adopted it, from choice. Far from being ashamed, I am proud to acknowledge, that I have often BORROWED from my venerable Preceptor; but I think it necessary to make this confession, that I may not be accused of having STOLEN from him.

† Practical Enquiry, p. 176.

“modic

“ modic constriction of the bronchia has been discovered by dissection, it is fair to conclude that it “ was not a part of the last condition of the lungs,” does not necessarily follow from your premises, any more than if it should be said,—as no proof of singultus can be demonstrated after death, it is impossible that a dying person could have sustained hiccough.

Indeed, Sir, you seem to have entertained some doubt of the force of your own argument, when in another place you remark, that “ though circumstances* will not encourage the assumption of “ spasm of the bronchia as a part of the paroxysm “ of Asthma, it is not intended to deny positively “ this condition.”

But, while I only object to the weight of your arguments in opposition to the theory of spasm; I request that I may not be misunderstood, as contending in favour of Dr. Cullen’s hypothesis, of which I always thought it incumbent on him to have adduced more substantial proofs, than any which have yet appeared, in support of it.

It is the bounden duty of every person who makes an *assertion*, to exhibit proof of its *truth*, instead of trusting its establishment to the precarious reception of credulity, or to the lame and il-

* Practical Enquiry, p. 170, 171.

logical defence, so frequently set up, that *the contrary* cannot be proved.

In speaking of the sensation of "straitness," or, as it may be termed, obstruction of the chest, in the paroxysm; you observe—"The vesiculæ* are occupied by lymph, the bronchia are straitened in their area by the same cause, in a more viscid state, the diaphragm cannot descend as it used to do, on account of the resistance of the stomach, filled and distended with flatus, and the trachea is probably narrowed by the compression of the œsophagus, affected like the stomach."

It is extremely unfortunate, when a careless expression happens to find its way into a book of science, which has a tendency to mislead the reader, on a point of great practical importance. I am far from attributing to you, Sir, a design to convey any other, than your most correct ideas of the state of the organs you mention: but as erroneous conceptions are sometimes produced in the minds of students, by loose and popular terms, I have some apprehensions of a similar inconvenience from your remark of the stomach being "*filled and distended*;" and my fears are increased by a coincidence of expression, in another part of your "Practical Enquiry."

* Practical Enquiry, p. 183, 184.

It is of the utmost consequence that a fact, at present too much neglected, even by practitioners, should be generally known, and attended to. I mean, that when even the smallest quantity of food has been received into the stomach, it is properly to be considered in a state of plenitude, equally, whether that quantity be an * ounce or a quart; for although capable of great distention, it is always applied to its † contents.

But the expressions of which you have made use, appear *primâ facie*, to support the vulgar and erroneous idea that the stomach, like a bag, may contain a certain measure *without* being full; and although it cannot be supposed that you *intended* to be *so* understood, it unfortunately appears that you have, a second time, countenanced the same notion, by speaking of the stomach being “full ‡ or empty”, in avowed contradistinction to excessive distention, on the one hand, and to perfect (that is, *real*) emptiness on the other.

As to the *probability* of the Trachea § being narrowed by the pressure of the œsophagus, when distended with flatus, which I perceive is hinted at,

* Fordyce's Treatise on the Digestion of Food, p. 13.

† Dr. Browne Langrish's Croonean Lectures on Muscular Motion, p. 46.

‡ Practical Enquiry, p. 251.

§ “Flatulence may force the sides of the œsophagus against the trachea, compressing the cavity of that pipe into a smaller area.”

Practical Enquiry, p. 266.

in different authors, it seems to rest on a very precarious foundation: for whoever will take the pains of making the experiment, will find it *impossible* to compress the hard cartilaginous rings of the larger branches of the Trachea, by the force of any distention, of which the œsophagus is capable;—not even if it be filled with a weighty fluid.

The wheezing, and the sensation of obstruction may, however, be satisfactorily explained, without resorting to such an hypothesis: and indeed you have yourself accounted for it, in a different manner,—by the “secretion * of mucus”, and its accumulation in the bronchia.

Whatever degree of hesitation I may have felt, in reviewing your observations relative to the stomach; I have no excuse to offer for criticizing upon the vague application of another term, whose importance in medicine should prevent its being trifled with, or applied incorrectly.

* Practical Enquiry, p. 309. It is to be remarked, that this SUPPOSED distention of the œsophagus is said to be “productive of wheezing where there is NO OBSTRUCTING MUCUS in the air passages sufficient to account for that symptom” (Practical Enquiry, p. 266) and that you have afterwards observed, “Wheezing is NOT a symptom, unless the long continuance of the fit should induce a secretion of MUCUS SUFFICIENT TO STRAITEN the bronchial passages;” to which you (I think UNFORTUNATELY) add, “I prefer PROBABLE CONJECTURE to deranging the induction established in a great majority of cases, by assigning to SIMILAR effects, causes DISSIMILAR.” Practical Enquiry, p. 310.

You

You say, "Fever * is the system of morbid actions, which in Catarrh as well as Phthisis is adopted by nature to relieve the body of those affections."

Instead of considering it in this light, it has obtained, in my mind, a much higher rank in the classification of diseases.

Fever is a disease, of which all the learning of the Greeks, all the industry of the Arabians, and all the boasted wisdom of the Egyptians, were insufficient to afford the Practitioners of those nations any accurate conception. That this term should, therefore, have been applied incorrectly, or even at random in all ages, and in all nations, is by no means wonderful, when we reflect on its original signification having been derived from the idea of heat; and recollect in how vague and diffusive a sense that idea has been, and is entertained.

But, whoever regards with attention, the usual operations of nature, or, in more correct language, the wisdom of the Almighty in the organization of animal bodies, will immediately be sensible, how very erroneous have been the conceptions formed, respecting a disease whose † prevalence at all times,

* Practical Enquiry, p. 191.

† "It may therefore be supposed that the history of fever should have been very perfect by this time, since it is one of the most frequent diseases, and has been in all ages, and in all countries, more especially

and in every country upon earth, has rendered it one of the most important in the history of medicine, and, at the same time, has afforded a constant opportunity, of its real nature being perfectly and completely understood.

That, at the termination of the eighteenth century, however, and aided by all the modern assistances, not merely of the improvements in Chemistry and Anatomy (which have contributed but little to perfect our knowledge of the nature of Fever) but by the important experience of clinical Practitioners, who have illustrated the history of the disease, by indefatigable researches, and the most patient industry:—that with all these advantages we should still, blindly and implicitly follow the erroneous doctrines of ancient theorists, or even adopt their modes of expression, betrays a degree of professional infatuation, which I hold to be utterly unworthy of this enlightened and philosophic age: for we may truly apply to the errors and prejudices of *our* early writers, what has been said, with great force, on a different occasion—“*Multa dicuntur juxta opinionem illius temporis, et non juxta quod rei veritas continebat.*”

Nothing has been more conducive to the establishment of erroneous opinions respecting fever,

“ally as it is likewise one of the most fatal; and as it so occupies the whole system as to absorb, during its continuance, all the faculties, both of the body and the mind, in a greater or less degree.”

Fordyce on Simple Fever.

than

than the idea so obstinately adhered to, that it was a natural effort to throw out some *noxious matter* from the body; an idea which has descended from Hippocrates through innumerable authors, and which really seems to pervade your “system of morbid actions adopted by nature to relieve the body.”

I am aware that this has been the prevailing opinion of many *modern*, as well as ancient practitioners; and that it has been maintained with the most zealous obstinacy,—in spite of absolute want of proof, and in opposition to every day’s experience. The whimsies of Paracelsus, and the dreams of Van-Helmont were not more absurd. But although there has never been produced the shadow of proof, what the nature of this noxious matter, or turbulent spirit, might be; the disease, properly called Fever, has been found to arise in thousands of instances, to go through its whole course, to be complete in all its stages, and at length to terminate,—sometimes in health, and in other cases, in the destruction of the body: wherefore, Fever is a distinct disease, and NOT *an effort of nature to carry off* matter whose retention would be prejudicial; and if Fever be a distinct disease, and not an effort of the system to overcome a subtile spirit, or to concoct * grosser matter, (which has never been

* Cheyne’s English Malady, p. 201. See also Sydenham; and ALL the Ancients,

proved

proved to exist) it necessarily follows, that in those cases, where a disease, arising from the presence of sensible and demonstrable matter of any kind (as for example, of mucus in Catarrh) produces a particular state of the body, which is to carry off the original disease, that particular state, so, and for such cause produced, is NOT *the disease properly termed* FEVER.

Whenever, therefore, the term Fever be employed in a signification so widely different from its *correct* meaning, it ought to be accompanied by a proper elucidation, that the mischiefs which might result from the conveyance of an erroneous idea may be prevented; and that the reader may know in what sense the expression is to be understood*.

You have mentioned Sir John Floyer's error†, in supposing that *fever* occurs during Asthma; but,

• “ Nothing, indeed, has disgraced physic more, than the duplicity
“ with which terms are adopted, and causes of diseases described, which
“ were never authorised by fact.” Lettsom's Medical Memoirs, p. 3.

† “ There is sometimes in Asthma a remarkable sensation PER-
“ VADES the body, accompanying the anxiety of the breast, and distin-
“ guished by an OBSCURE perception of Heat, and A WANT of cool
“ air. This perception of heat may have induced Floyer TO TALK
“ OF FEVER IN this disease.” Practical Enquiry, p. 215.

The former part of the sentence is indeed A LITTLE OBSCURE; but it may be fair to apply to it your remark on Willis, “ the LAN-
“ GUAGE is perhaps MORE in fault than the conception :” and this may also be extended to another assertion, viz. that “ in Asthma, the
“ system is INVIGORATED with TOO SMALL a proportion of oxygen.”

Practical Enquiry, p. 241.

I must

I must take the liberty of observing, that you do not appear to reject it, on what I conceive to be the proper ground:—for you afterwards remark, that “an attention * to the pulse gives no countenance to the observation;” because “the quickness never amounts to one hundred beats in a minute.”

But surely, Sir, it is not your intention to assign the degree of celerity of the pulse, as the criterion of the presence of Fever! Were this the case, every man who rides a hunting, on a moderately warm day, might be said to be in a fever. Every person, the passions † of whose mind were suddenly and vehemently excited, might be called feverish: rage and fear, love and jealousy, might then be denominated fever: as well as an infinite catalogue of diseases, which are not at all allied to it, in their nature or essence:—which neither accord with the symptoms of that disease, now properly termed fever, nor even with that which was considered as such, in remoter ages, by the physicians of Greece and Arabia.

* Practical Enquiry, p. 215.

† Fordyce.

In the preface to the second volume of *Zoonomia*, Dr. Darwin has thought proper to employ the word *FEVER*, as descriptive of a “quick pulse which continues for some hours.” I have before observed, that every author has a right to make use of his own terms: but *NO AUTHORITY*, excepting that of just reasoning, shall compel me to accede to the propriety of their application. It should be remembered, that quickness of the pulse is not *NECESSARILY* present, to constitute the disease, properly called Fever.

F

In

In the ninth section of your "Enquiry," I meet with the following passage: "In Asthma*, an *excess* of blood, in the pulmonary vessels, may very probably precede the exhalation of the finer part into the vesiculæ and bronchia; this plethora arising from the relaxed texture of the coats of the vessels," and concluding with "blood letting is *never*, to my †knowledge, useful, but frequently injurious."

When "an *excess* of blood" prevails, it appears somewhat extraordinary that blood-letting should not be useful, by lessening the degree of distention, which assists in straightening the bronchial vessels, and concurs with the effused serum to diminish the sum of their cavities, at the same time, that it tends to destroy the contractile power of the blood vessels themselves.

It is not worth while to combat the idea of "*Plethora, arising from the relaxed texture of the coats of the vessels,*" which is an assertion not to be

* Practical Enquiry, p. 192.

† Practical doctrines are, undoubtedly, recommended with considerable effect, by the authority of PERSONAL observation: but how unfortunate is it, that the experience of one author is so frequently contradicted by that of others! Not to mention, perhaps, hundreds of instances in which LIVING Practitioners have seen the beneficial effects of BLOOD-LETTING in the paroxysm of this disease,—on looking into "WILLIS, I find, that in one of THE TWO CASES with which he has illustrated his remarks "*de Asthmate,*"—he expressly mentions the patient, (a nobleman, if that confers any weight on the history) being RECOVERED FROM THE JAWS OF DEATH by phlebotomy.

defended

defended by any argument deduced from reason or physiology: because it has not yet been proved that the coats of the vessels *are relaxed*, unless in those cases of Asthma which arise in, or are succeeded by, an anasarcaous habit. This subject I am not, however, at present, to discuss.

But, Sir, when you admit that Plethora does exist; when you allow that “there* are certainly “striking circumstances of analogy between Asthma “and Apoplexy;” when *you agree* with Dr. Cullen, that an accumulation of blood in the vessels of the brain, and an effusion of serum, occasioning compression of that organ, are the proximate causes of apoplexy; and have grounded on that opinion, your remark, that “these † states applied to the vessels “of the lungs will be found to induce Asthma;” you must permit me to express my astonishment, that, in opposition to *such doctrine*, you should discountenance phlebotomy. But you have gone further, and remarked, that “there is *reason to apprehend*, an injudicious use of the lancet in pulmonary complaints, has *brought on* Asthma in some “instances‡.”

As I differ from you respecting the cause of Asthma, it will not be surprising that I should not

* Practical Enquiry, p. 196.

† Ibid.

‡ Practical Enquiry, p. 296.

concur with you, on the above mentioned point: but, to enter into an argument about it, until I understand *the reason* of your apprehension, would be like fighting a shadow.

It may be thought captious to dispute about an author's *belief*, when it is not productive of any suggestions likely to influence his readers; I will not, therefore, attempt to shake your *belief* of the *pneumonic inflammation* attendant on *intermittents*, and will only remark, that some men are extremely *prodigal of their belief*, and can scarcely withhold it from "every wind of doctrine," or every *rumour of absurdity*:—but there are others of a contrary disposition, who will hardly believe the best established facts, supported by the most respectable evidence. Freedom of belief, as well as of opinion, is happily still the possession of Englishmen; and is under the dominion of conscience alone.

In your enumeration of the symptoms which take place in a paroxysm of Asthma, an "*itching of the skin*" is mentioned, and attributed to "the causes which more peculiarly occasion the anxiety* and straitness of the chest,"—to "irritating matter in the first passages,"—to "acrimony which is to be discharged on the skin,"—to "sympathy†,"

* Practical Enquiry, p. 214, 215.

† This favours a little of the manner of the ancient writers.—"Fit singultus à repletionē, vel inanitionē, aut ab acerrima materia, vel quod contagione a nervosis partibus in sympathiam ventriculus trahatur, vel a vicinis, ut ab hepate."

and lastly, to “the difficulty with which venous
“blood is returned to the heart, from the right
“ventricle and the pulmonary artery being obstruct-
“ed.”

“In * the multitude of counsellors there is
“safety.” In this numerous assemblage of causes
there is at least some chance of being right. But
I must take the liberty of reminding you of your
own quotation from Dr. Crauford, “by † the rules
“of philosophizing, we are to admit no more causes
“of natural things than such as are both true and
“sufficient to explain the appearances,” and of the
subsequent remark‡, which, if attended to, might
possibly have directed to a plain and obvious indica-
tion that would have prevented the fruitless labour
of “*speculating in complex causes.*” But so true is
the axiom, elegantly conveyed in poetic language:

———“There are who teach,
“Truths they not feel, nor practise what they preach§.”

* Proverbs, chap. xi. v. 14.

† Exper. on Anim. Heat, p. 366, quoted by Dr. Bree.

‡ “Nature does nothing in vain, and having exhibited to us any
“one cause of an operation, we are not fruitlessly to speculate on com-
“plex causes of that effect.” Practical Enquiry, p. 85.

§ The beautiful little poem, from which these lines were quoted,
(the composition of Mr. WESTON, of SOLIHULL) is a deserved pane-
gyric on the public virtues, and private worth of the Reverend JOHN
JAMES, Rector of PACKINGTON, in WARWICKSHIRE; of whom
it might be sufficient to say, that he is justly entitled even to the polish-
ed

Certainly, however, it is more commendable, for us to content ourselves with reasoning from effects*,

ed praise of Mr. WESTON's pen: I will, however, add,—that this good and venerable man has fulfilled every duty of a diligent and faithful Pastor, for nearly half a century; and so entirely possessed himself of the love and admiration of his parishioners, that all those jealousies and animosities, all those bickerings and altercations which disturb the harmony of society, and not unfrequently, end in ruinous litigation, are instantly subdued by the influence of his mild mediation. Eminently distinguished in the desk and the pulpit; he is equally celebrated for suavity of manners, evenness of temper, and judicious liberality.

“Amid the fold who thy protection share,

“Does one, one lurking ill elude thy care?

“Does lacerated friendship mourn? does grief,

“Or age, or want, or sickness ask relief?

“The good Samaritan allays the smart,

“Binds up the wound, and heals the broken heart!”

WESTON'S POEMS.

While I am thus adding my feeble tribute of applause to the general voice of that neighbourhood, of which Mr. JACQUES is so brilliant an ornament, I am tempted to digress still further, by introducing the name of the illustrious EARL of AYLESFORD, founder of Packington church. In evil days (like the present) a bright example cannot be too often nor too highly held up to the admiration of mankind. A Peer of the Realm, who confers honour on his illustrious birth and exalted rank, by virtues “all his own,” is not so commonly to be met with, as were “most devoutly to be wished.”—But when it is generally known that this nobleman (noble in every sense of the word) bestows that time and attention on elegant amusements, and works of benevolence, which so many OTHER LORDS devote to the race-ground, the gaming-table, or to the celebration of the nocturnal orgies of folly, dissipation, and vice; the torrent of democratic slander, and phlebeian abuse of the aristocracy, must be, in some measure, stemmed. Would to GOD that such a pattern of affability, integrity, charity, and piety, were generally imitated by the great!—The perturbed spirit of insubordination, which now stalks through the land, “making night hideous,” would then be charmed to rest! and the hydra of sedition, with all its forked tongues, would hiss in vain!

* Lewis Cornaro's Treatise on a Sober and Temperate Life, p. 81.

with

such as they appear, and with faithfully recording the symptoms of a disease, than that we should hazard the propagation of error, by *attempting to account for all* those effects and symptoms.

I am again so unfortunate as to differ from you on the cause of the paroxysm taking place in the night. Every one knows that when the respiratory muscles have been for some time at rest, the serous effusion into the vesicles of the lungs is increased, and that it will "create * more embarrassment as "its bulk extends:" but you are pleased to remark, that "volition † being suspended, the powers of "sensation and irritation are more active;" and quote Dr. *Darwin's* assertion, "Our irritability to "internal stimuli; and our sensibility to pain or "pleasure is not only greater in sleep, but increases "as our sleep is prolonged,‡" from which you infer, that the increase of sensation and irritation occasions the effort to expel the offending matter.

If, however, I shall disprove the premises, the conclusion which has been deduced from them, will necessarily fall to the ground.

Great names should not deter rational creatures (although not possessed of the transcendent abilities

* Practical Enquiry, p. 222.

† Ibid.

‡ Zoonomia, vol. 1. Sect. xviii. quoted by Dr. Bree.

of the celebrated author of *Zoonomia*) from a careful investigation of the doctrines delivered to us, even by authors of the most distinguished talents, or the highest professional celebrity. Error has been frequently sanctioned by the reputation of great men; but it still continues to be *error*, unchanged in its nature and effects, by the most general reception which the world may have been induced to give it, in consequence of a too ready submission to the dogmas of authority.

If our sensibility be increased during sleep, the mind should seem not only to be perfectly awake when the body is at rest, but to be more active in all those functions which enable it to participate in the delight of refined sensations.

This reasoning being applied to the immediate object of your enquiry, is thought to prove that the difficulty of respiration, which occurs at the commencement of the paroxysm of Asthma, is occasioned by the irritation of the offending matter being more acutely perceived, at that particular period, when this effect takes place.

Let us reflect, for a moment, on what happens during the state we call sleep; a state marked by the absence of volition.

In sleep, the judgment is undoubtedly sealed up, and remains entirely dormant*; for the utmost

* Fordyce,

absurdities of the imagination are allowed to wander without restraint, and the most discordant ideas are jumbled together, without any interference of the judgment to correct them.

Prolongation of time, and distance of place, are alike disregarded: and a person may dream of a combat of Roman Gladiators in the drawing room at St. James's; or of Æsculapius in the shape of an owl, gravely seated at a Gullstonean lecture, without any effort of the judgment being aroused, by the absurdity of the idea. But this is not the case when we are awake; the judgment *then* revolts* at such incongruities, and although the sportive activity of the imagination may present fallacious pictures, or heterogenous combinations of objects, the judgment instantly rejects them.

The imagination may be excited, either by the perception of stimuli, or by the aid of memory. Memory, which has been defined "the † power of recalling ideas which have been formed by impressions made on some of the organs of sense," may, itself, be aroused by a new perception, arising from an impression, similar to what has before called it into action; and, as it is capable of being acted upon, by *every* new combination of perceptions, which the imagination may effect, it neces-

* Fordyce.

† Fordyce's Third Dissertation on Fever, p. 28.

farly follows, that its action is, at least in some degree, regulated by that operation; which itself depends on the impressions conveyed by the corporeal organs of sensation, and on the ideas which have been laid up in the memory.

The imagination and the memory then, concur by an united action, to unfold and to prepare the ideas arising from the perceptions which the former receives from the organs of sense, and which the latter has treasured up; in order that the judgment may compare them together, and determine or decide on the propriety of their arrangement. The imagination resorts to the memory for a set of ideas, which the latter has the faculty of recalling. The memory assists the imagination in distributing, changing, separating them, and in recombining them after they have been separated. The memory and the imagination, therefore, may act, without * the judgment; although the judgment cannot exert itself without their assistance.

It has been admitted that the memory may act by its own inherent powers, but my present design is to consider it, as called into exertion by the perception of new ideas, in consequence of, and the cognizance of sensations resulting from the application of *external* stimuli.

* Fordyce.

The stronger, or more active the stimulus be, the force of its impression will be proportionable; and the remembrance of it the more distinct. Great pain and great pleasure, with all their concomitant ideas, sink deep into, and are firmly engraven on the tablet of memory. *The acuteness of original perception, therefore, may be traced by the faithfulness of memory, which is evidently influenced by it.*

If Dr. Darwin's assertion be understood literally,—that our sensibility to *internal* stimuli is increased during sleep;—that, as the memory does sometimes act, independent of any excitement *from without*, and as the operations of the imagination, during sleep, are uninterrupted by the exertions of the judgment, the sensations of pleasure or of pain *so* produced, are *more acute* then, because they are **unrestrained*;—in this case one might be not a little surprised, that such a remark should have been adduced, in order to support the opinion, that *when volition is suspended, our perception of serum accumulated in the bronchia is most acute*. But notwithstanding the *literal* meaning of the words “internal stimuli,” and the further elucidation of that expression, in the second volume of *Zoonomia*, where the learned author says, “in † delirium, reverie, and sleep, the power of perception is *abolish-*

* Lord Monboddo's *Ancient Metaphysics*, vol. 2. book iv. C. 4 & 5.

† Darwin's *Zoonomia*, vol. 2, p. 141.

“*ed;*” and mentions “Incubus,” as probably
 “owing * to the irritability of the system being *too*
 “*small* to carry on the circulation of the blood
 “through the lungs, during sleep, when the volun-
 “tary power is suspended:” it may be presumed,
 from other expressions, such as that of “the system
 “being more sensible † and more irritable during
 “sleep,” and a “perpetual ‡ increase of sensibility
 “during the continuance of sleep, as in some instan-
 “ces of epilepsy, *asthma*, &c.”—that you have
 Dr. Darwin’s authority on your side, to support
 a theory, or rather an opinion, which is contradicted
 by all || experience, and by almost every metaphy-
 sical writer, from the days of Aristotle and Plato in-

* Darwin’s Zoonomia, vol. 2, p. 400. Whether the doctrine here advanced be true or false, is not the object of my enquiry, the reference being made only to prove the REAL meaning of Dr. Darwin’s expression respecting the INCREASE OF SENSIBILITY DURING SLEEP.

† Darwin’s Zoonomia, vol. 2, p. 399.

‡ Ibid. p. 515.

|| Shall it be asked ? How is it possible for a suspension of the power of volition, to DIMINISH the velocity of the circulation through the lungs ; and, at the same moment, to increase our sensibility of, and consequently our efforts to expel, THE OFFENDING MATTER ? which are manifestly contrary effects. It is true, Dr. Darwin has SAID that “the system becomes more sensible, and more irritable during sleep ;” and in another place, that the degree of irritability, which exists when volition is suspended, is insufficient to carry on the circulation with its accustomed energy : and in another part of his work, that “in delirium, reverie, and sleep, the power of perception is ABOLISHED.”

If the irritability be too small to maintain the circulation in its usual force, how can it occasion an INCREASED effort to expectorate the accumulated mucus ? In short, how are the above contradictions to be reconciled ?

No

clusive. Neither his authority, however, nor that of all the Physiologists in Europe, shall make me acknowledge what is so directly contradicted by every one of the senses; nor will I ever consent to believe, that the wise Author of our being only sent us into the world *to dream*, until the advantages which result from such an increase of sensibility during sleep, shall be more evidently demonstrated.

Can it be supposed that our perceptions should have been rendered most acute, at the time when we are least capable of arranging, or of enjoying the arrangement of the ideas which they excite? or, that the wandering pleasures of an uncontrolled imagination, full of absurdity; and the wild excursions of memory, without connexion, should have been bestowed on us, as the supremest gratification which we are able to feel? Had this been the case, the martyrdom of the seven sleepers, said to have been shut up in a cave at Ephesus for more than three hundred years, would, after *so much pleasure*, have been almost enviable!

It is, therefore, *unreasonable to suppose* that our sensibility is increased during sleep; and the *belief* of it may be proved to be erroneous, by the following facts.

No authority should be regarded in medicine but that of just theory, founded on real observation. No consequences should be admitted † which are not immediately derived from actual experience, and no submission is due to nominal influences.

† Lavoisier's *Traité Elementaire de Chimie*, vol. 1.

A great

A great proportion of our ideas may be said to originate through the medium of sight, and unless the mind be very attentively * engaged, the objects painted on the retina make some degree of impression. It is not necessary that this impression should continue for a long time, in order to its being retained *in the memory*: for a comet may be seen for a moment, and the perfect image of it never be effaced from our recollection. It is only necessary that the mind should be *capable* of being fully impressed at the time.

A complete picture may be formed, consisting of many objects;—these are doubtless, all of them, represented on the retina at once; but the mind is only sensible of the most striking features, and its attention being engaged by them, *all* the several objects are not conveyed to the memory; and the recollection † of many of them is consequently lost.

In sleep, the eye-lids are usually closed, but not always; for some persons sleep with their eyes wide open, and in broad day light. Volition being suspended, the muscles of the eyes cease to accommodate themselves ‡ to the distances, at which objects are most distinctly visible; but there are

* Fordyce.

† Ibid.

‡ Dr. Alexander Monro's Treatise on the Eye, p. 137.

Dr. Brandish's Dissertatio Inauguralis de Visu, p. 39.

Reid's Inquiry into the Human Mind, p. 240.

some objects occasionally brought to the focal distance*, and yet they will not be found, once in a million of instances, to excite the smallest degree of perception. Nor is any idea produced by the *consciousness* of light, which, when we are awake, accompanies our perception of objects, and is present when the retina is only stimulated by the rays of light falling on it:—the eye being placed in a situation in which the reflexion of other objects cannot possibly reach it. We may, indeed, *dream* of perceiving the impression of light, but this is an act of memory † alone, nearly resembling that state of ideal insanity ‡ in which a patient imagines he sees or converses with persons, who have no external existence to his senses at the time.

Even if objects, at the focal distance, were *visible in sleep*, it would be no proof that the powers of perception *are then increased*: but as the diminution of sensibility in the optic nerve always accompanies the quiescence of the voluntary muscles, this is an additional argument against the doctrine advanced in your “Practical Enquiry.”

If we look with attention at the structure of the ear, we shall be convinced, that although the sense of hearing be termed *involuntary*, there are parts of that organ which assist in the conveyance of sounds

* Fordyce's third Dissertation on Fever, p. 34.

† Fordyce.

‡ Arnold's Observations on the Nature, &c. of Insanity.

to the auditory nerve, evidently controllable by the powers of the will*, which does not merely render *them* more distinct, by abstracting our attention from other impressions, but manifestly contributes to increase their effects, by exciting the action of those muscles which fit the meatus auditorius for the primary reception of sounds, and at the same time, adapt the membraneous part of the case† which contains the vestibule and semicircular canals, to the impressions about to be made on the last named organs.

Many sounds, undoubtedly, enter the ear while we are awake, of which the mind takes no cognizance; but it surely will not be contended, that we *bear better when we are asleep*: nor will it be said that the shepherdess, reposing on a bank of violets and honeysuckles, enjoys the *smell* of their delicious perfume, the more sensibly during her slumbers.

Epicures may sometimes be gratified even by *dreaming* of turtle and venison; but if, by your means, the opinion that their *taste* is improved during sleep, were once established, it would operate as an anodyne to the whole Court of Aldermen; who would *gape* at the name of Bree,

“thenn nodde ther thankes and falle asleepe.‡”

* Darwin.

† See an excellent description of these parts, by Dr. Monro, in his Remarks on Professor Scarpa's Book on the Ear, p. 239.

‡ Chatterton's (or Rowley's) Account of Cannyng's Feast.

Another

Another organ of perception is the skin: and will any man in his senses pretend to argue, that the effect of Cantharides, or of any other irritating application*, is felt *more acutely during sleep*? To come still nearer to the point; the accumulation of water in the painful ascites, and in dropsy of the chest, and in hydrocele, is certainly not productive of any increase of distressful sensation during sleep: neither are abscesses and tumours, nor is the sense of distention in cases of aneurism, or varicocoele, felt more acutely at that time.

These remarks might be extended to a great length, but you will *perceive* that I think it unnecessary to advance further proof. The experience of every person in the world being found positively to contradict this *imaginary* increase of sensibility; I assume, then, that the fluid accumulated in the lungs, during asthma, is *not* felt with increased sensation during sleep, but only in proportion to its increase of quantity; and that *the powers of sensation and irritation are NOT more active* at that period.

* My much esteemed friend, the late H. M. Esq. representative in parliament for Bosciney, in Cornwall, having accustomed himself to read till a very late hour, after he retired to his chamber, happened once to fall asleep during this employment, with a candle in his hand, which, being soon melted by the fire, fell on his leg; and the wick following the wax, continued to burn until his stocking was consumed, and a considerable portion of skin perfectly coagulated by the heat, so as to form a large eschar, before the sensation of pain (although the stimulus must necessarily have been acute) aroused him from sleep. Many similar, and, perhaps, more striking examples, might be produced, if necessary.

H

Your

Your observation on the beneficial activity of the lymphatics, in the removal of offensive matter from every part of the system, (supported by the coincidence * of Dr. Darwin) having induced you to reject an opinion that “a *paralytic* † *atony*” of that system is one of the causes of the accumulation of fluid in asthma; I shall not introduce any further remark on that subject, than a hint that some pains appear to have been taken, to disprove what had never been established: for, (notwithstanding Dr. Richter’s ‡ Theory of Dropsy) there is no more *evidence* || to support the doctrine of “a *paralytic* “*atony*” of the lymphatics, in *uncomplicated* asthma, than to prove the existence of the man in the moon.

As to the puerile idea of the elective **, or discriminating power of the lymphatics, it is so directly repugnant to every principle of true reasoning and physiology, as scarcely to deserve the trouble of refutation; although I have heard an anatomical

* Darwin’s Zoonomia, vol. 2, p. 163.

† Practical Enquiry, p. 273.

‡ A German physician, and author of a work entitled “Medical and Chirurgical Observations,” translated by Dr. Spens.

|| Let it be remembered that Dr. Darwin only *SUPPOSES* that the Paroxysms of asthma depend on what he calls “torpidity of the absorbents,” and acknowledges that their “cause is not WELL understood.”
* Zoonomia, vol. 2, p. 520.

** “It takes up by *SELECTION*.”

Practical Enquiry, p. 276.

lecturer gravely expresses his astonishment at the *wonderful* alacrity with which these vessels take up nutritious matter, and refuse that which is noxious ! Where is this *wonderful sagacity*, when they suffer themselves to be gorged with a mixture of quick-silver and grease, which they imbibe with as much avidity as milk and honey, or the chyle which supports their very existence ?

I pass over the particulars of your division of Asthma into *four species*, the necessity of which, will, I presume, be superseded, when I have described the *nature* and *cause* of that irritation, which, *in every species* of the disease, you have admitted to exist.

The earlier writers, it is well known, considered Asthma as a pituitous affection, which was afterwards divided into two species, and termed Humoral and Nervous. The first, they described as occasioned by a *defluxion* * of serum from the head, and the last has been since called a spasmodic effort.

And although the ancients either “boldly waded “in darkness†” or were led into endless error by the glare of false theory ; they saw plainly that a quantity of mucus was to be expectorated, and,

* “ Congeritur in capite pituita, quæ labitur in imbelles jam pul-
“ mones.” J. Heurnii in lib. iii Aphorism. Hippocratis, p. 210.

† Darwin's Zoonomia, Preface to Vol. I.

therefore, endeavoured to promote this operation; leaving the *real cause* of the accumulation, to be discovered by the industry of future ages. The methods employed were certainly different from those which a modern Physician would recommend; and, perhaps, some of their remedies were made use of without much reflection or study. Thus, Emetics were prescribed by *Riverius*, although he certainly did not consider the irritation of the stomach as in any degree connected with the disease. *Zacutus Lusitanus* used Diuretics, but it is not very clear that he had the least idea of depriving the blood of any principle which at all conduced to the production of Asthma. *Willis*, it is true, ordered expectorants, (as *Horatius Augenius*, *Sennertus*, and *Riverius* had done before him) and appears to have had a confused notion of what he called a *serous humour* in one place, and a *vapour* in another, existing in the blood: he undoubtedly used expectorants because of the mucus in the bronchia, but it is not so easy to determine his reasons for that elegant addition, prepared from snails and earth-worms *.

* Snails and Earth-worms, however, as well as ALL THE REPTILES of the earth, have had their advocates, and even admirers; and I am not ignorant, that many and great virtues were formerly ascribed to them; of which a very pompous record has been made. “Infig-
“niter diuretici sunt, diaphoretici, anodynî; discutunt, emolliunt,
“obstructiones referant, lac augent, vulnera nervosque præcisos glu-
“tenant. Usus præcipui sunt in Apoplexiâ, Spasmo, allisque affec-
“tibus nervorum ac musculorum—In iëtero, hydrope, dolore colico.
“Imprimis autem specificè profunt in arthritide scorbuticâ, podagros
“dolores leniunt.” Dale’s Pharmacologia, p. 530.

More

More modern practitioners have described the particular condition of the body, in which Asthma makes its appearance : and, from an attentive consideration of it, have discovered, that, in order to prevent the recurrence of the disease, that state of the system in which it has been most frequently observed to arise, must be prevented.

Hitherto, however, I believe the manner in which that condition of the body, which has been called predisponent to Asthma, really produces the disease, has not been explained, nor even attempted to be explained, by any author who has expressly written on disordered respiration. It is amusing to trace the regular, but often, slow progression, by which genius is conducted, from one discovery to another, and so on ; 'till suddenly she shall sit down, as if impatient of a resting place, at the very threshold of some important improvement ; contented with the gleanings she has already met with, and seeming to disdain the trouble of further investigation.

In later times, Practitioners having acquired a correct idea of the arterial system, and finally of the lymphatics, were no longer at a loss to determine in what manner the serous effusion, which obstructed the bronchial tubes, was deposited in the lungs ; nor to account for the invisible removal of it, in various instances ; and the translation of
the

the disease, or rather of this symptom of it, to some other cavity.

They have recorded the appearances; they have observed the changes which take place in the system, at the time of the occurrence of the disease; and at the periods of its variation: they notice the irritating effect of the effusion, on the fine membrane which lines the branches of the trachea; they include the disorders of the first passages, among the causes which induce such a state of the body, as particularly fits it, for the attack of the paroxysm; they even mention the *acrid* quality, as well as offending quantity, of the effusion; yet not one among them, not even you yourself, have thought it expedient to enquire, what is *the nature* of this irritating quality, in the serum? and what the means by which it is produced?

You admit, Sir, that the influence of this principle is so powerful, as to be capable of exciting violent spasmodic contractions of the muscles concerned in respiration, and do not so much as hint at the probability of this irritating quality having been acquired, subsequent to the exudation of the serum: it may be presumed, therefore, that you agree with preceding authors in supposing such quality to have existed in that fluid, previous to its being expelled from the blood vessels: and yet, you have not thought it worth while to enquire into its properties, and the manner in which it is formed!

Viewing

Viewing the subject in this light, I am almost induced to suppose, that they, who have trodden in the same path before me, had become weary, by the fatigue of their researches, and abandoned the pursuit, after having followed the object of their enquiry, through its greatest intricacies, and chased it into open day light.

The proximate cause of Asthma, is allowed to be a serous effusion in the pulmonary vesicles, offending, either "by * its oppressive bulk, or acrid quality, or both."

Hence it may be inferred, that you do not attribute the offence to the stimulus of *quantity*†, simply considered as such; because, it may be produced, when no evident proof of the presence of gross matter can be demonstrated: and we can scarcely conceive how an effect may be occasioned by quantity only, when it is so minute as to be imperceptible.

* Practical Enquiry, p. 318.

† "The second species of irritation is the acrid or offensive *QUALITY* of some *UNOBSERVED* matter, *CONVEYED BY THE AIR*, "and attached in the act of inspiration to the *SENSIBLE* membrane "lining the trachea and bronchial pipes." Practical Enquiry, p. 308. Two obvious questions arise out of this short sentence: first, What proof is there of this *UNOBSERVED MATTER* being conveyed by the air? Secondly, How is the *SENSIBILITY* here allowed to the membrane of the trachea reconcilable to *YOUR PRECEDING REMARK*, that "the lungs have little sensation," which has been already noticed in the fifth page.

The

The idea of quantity, connects with it that of space, or distention; when, therefore, an effect is produced by matter, which stimulates in a very small quantity, the mind refers that effect to a specific power of action different from quantity, which we call *quality*.

Nevertheless, it is necessary that a certain number of original points, or atoms, should be collected, in order to excite perception: but the increase of their number does not alter their quality, it only augments the force of their effects.

A mass of matter, uniformly red, being divided * into its minutest parts, loses its *sensible* colour, but every particle is still red, though it be too small to impress the organ of sight with that idea. A particle of red blood is not altered in its original quality by being divided, only, it no longer retains its usual appearance. If a mass of matter, by having its parts divided, were to sustain any alteration of its nature, or in another word, of its quality; it would also acquire *new* properties by an accumulation of size; but, a single grain of salt conveys a perfect sensation, not at all different, except *in degree*, from ten thousand grains; for an original particle of matter, must, for ever, continue † the same, and remain eternally unalterable in its essence; un-

* Treatise on the Digestion of Food, p. 125.

† Fordyce.

less the Almighty, who created it, shall be pleased, miraculously, to give it different properties, or to annihilate it.

But matter, which, in a small quantity, is perfectly inert, and incapable of exciting the action of the living fibre; may, when accumulated to a certain degree of bulk, irritate by its power of distension. In this case, the effect may be said to arise from *quantity*.

To illustrate my remark. An ounce of warm water thrown into the stomach, does not stimulate it to contract, in any degree, of which we have the smallest evidence or perception:—but a quart, or, perhaps, a third or fourth part of that measure, will excite violent contraction. Here, then, the stomach is excited to action by the stimulus of quantity, or distension. It is evident that no new properties are, in this case, applied to the stomach by repeating the draughts of water; for, if the quantity, which is known to be sufficient to provoke vomiting, be divided into small portions, and swallowed at considerable intervals, although the same quantity will come in contact with the coat of the stomach, in the latter case as in the former; the same effect will not be produced. Neither is there any thing in warm water, which has a tendency to occasion the contraction of the living fibre:—it does not corrugate the skin when applied to it, and certainly does

I

not

not stimulate a muscle to contract, by any inherent property, notwithstanding it has been termed *a cordial**. Its effect, then, must depend on the distension it produces, partly by its mechanical bulk, and partly by the expansion which its heat may occasion, of the air, with which it happens to meet in the stomach.

Again, a particle of cantharides, or of volatile alkali, when applied to any part of the living body, stimulates it, and produces pain, or at least a sense of heat; but certainly not from its weight or power of distension. Many particles, will, it is true, produce a proportionably greater effect;—but in no way different, unless in degree, from that of a single particle. This effect, is said to arise from the peculiar quality, or property, with which these substances are endued.

In like manner a few grains of ginger, or pepper, being applied to the membrane of the nose, induce the effect called sneezing, which is an effect of quality, not of quantity; for an equal portion of starch, or of powdered chalk, might be so applied, and yet no such effect be produced. A person, indeed, accustomed to the use of snuff, becomes habitually insensible to the stimulus of the same quantity of it, which is capable of exciting violent efforts in ano-

* Boerhaave's Praxis Medicinæ.

ther:

ther : but this difference is manifestly occasioned by a morbid affection of that part of the membrane to which the snuff has been customarily applied; for any other living fibre of the same person may be irritated by an equally minute particle of stimulating matter, which would be required to produce an effect on another person; and he who is become insensible to the action of a particular quantity of any stimulant, may yet be affected by a greater number of particles of the same, notwithstanding it has been shewn that no difference in *the nature* of their action is occasioned by increasing their quantity.

Matter therefore which, in minute particles, is perfectly incapable of stimulating, does not acquire any *new properties*, in order to be rendered capable of exciting irritation in accumulated quantities.

Hence it appears,

- I. That matter perfectly incapable, in its nature, of irritating the bronchia, or of exciting contraction of the respiratory muscles, may be deposited in the lungs, without offending, unless by its quantity.
- II. That matter of an irritating quality may stimulate the vessels, in which it is contained, although it be present in a quantity so small as to escape detection by the eye.

In every case, in which a considerable alteration of the quality of the solids and fluids of the body is produced (and, as these are compound substances, they may suffer alteration, either by a variation in the proportions of their component parts, by being entirely deprived of one or more of them, or by having some new principle, different from any which they before contained, superadded to them) a certain degree of disorder, or derangement of their functions, is necessarily occasioned. Symptoms of such irregularity present themselves to an attentive observer, during life; and after death, an affection of the solid parts, exhibits on dissection, appearances which are called morbid; because they differ from the usual appearances of the same organs, when in an healthy state.

It is by an attention to the phenomena of these symptoms, during life, and of these appearances, after death, that we are enabled to discover the nature and the causes of those irregularities which constitute disease: and to adapt remedies suitable to their removal when present, and their prevention when absent.

It immediately strikes the most superficial observer, that the presence of a serous effusion in the lungs, is not a necessary consequence of their particular organization; and, therefore, that it must be occasioned by some derangement, either in that organ,

organ, or in those parts of the system to which it is subservient, or with which it is directly connected.

Anatomy has taught us the nature of the connexion between the lungs and the rest of the system. It is sufficient for my present purpose to observe, that the lungs are furnished with a set of tubes, called air vessels, (the structure of which it is unnecessary for me to investigate in this place) whose large trunk opens to admit atmospheric air, but is fitted with a * contrivance for the complete exclusion of any other matter;—and whose extremest ramifications open into vesicles which every where accompany the terminations of another set of vessels proceeding from the heart, and carrying red blood, with which the air, admitted by the bronchial tubes, is intermixed. This operation is perpetually going on, and the arterial blood, having received a portion of the atmospheric air, is re-

* The Epiglottis.

The muscles * which shut up the passage of the larynx are so strong† in many persons, that they can suffer a large quantity of fluid to pass down the throat, without any danger of the smallest particle being admitted into the trachea, although the mouth be kept wide open.

Custom undoubtedly increases the strength of this faculty, as appears in the example of some of the eastern nations, where it is a common custom to pour their drink into the œsophagus, without touching their lips with the vessel which contains it.

* Cheselden's Anatomy, p. 78, and subsequent.

Winflow's Anatomy, translated by Douglas, Vol. 2. Sect. 10.

† Cowper's Anatomy by Albinus—Tab. 5th of the Appendix.

turned

turned by other vessels, called veins, to the heart, to be distributed to the remotest parts, as well as the inmost recesses of the body, while certain particles, which may be termed excrementitious, having been parted with, by the blood, in exchange for pure air, are received into the bronchial vessels, and by them conveyed out of the body in the shape of vapour, which is vulgarly called *breath*.

Besides this vapour, the air vessels in a state of health, throw out no other matter: but in the disease termed Asthma, the muscles which alternately dilate and contract the cavity of the thorax, and fit the lungs for receiving the air, and respiring the breath, through the bronchial vessels, are violently irritated, and with laborious efforts expel, through the same tubes, a quantity of mucus, which, it is obvious, could only have entered into their cavities, through the vesicles and the minute extremities of the blood vessels.

From the size of their orifices, it is equally obvious that this mucus, at the time of its passing through them, must have been of no very considerable density: and that it had previously circulated in the blood vessels, either in combination with the blood itself, or in a separate state.

Innumerable opportunities of observing the blood of patients, labouring under the paroxysm of Asthma,

Asthma, having never afforded the least ground of suspicion, that this fluid had existed in the blood vessels separate from the blood; it necessarily followed, that it must have been combined with the blood itself, and consequently, that it was originally parted with, in the form of serum.

An attentive examination of the delicate membrane, which lines the branches of the trachea; and frequent experience of the distress, occasioned by any extraneous matter, even in the smallest quantity, being forced into them, together with the appearances on the dissection of healthy subjects, were sufficient to convince modern practitioners that the effusion discovered in Asthma, could only be derived from the arteries of the lungs; but it would naturally lead them to remark, that matter of the most unirritating kinds, and such as produced no stimulating effects whatever, in another part of the body, might occasion great irritation there, however small might be its quantity.

If then they had detected an acrid quality in the serum, effused into the lungs, it would have been difficult to say whether that organ might not have been irritated by the stimulus of quantity, rather than of quality.

But as no intermixture of the serum with any substances, but the substances of which the atmospheric air is composed, could possibly have taken place,

place, after its exudation in the lungs, if any acrid quality prevailed in that fluid, it must either have been obtained previous to its being deposited there; or have acquired it, by attracting oxygen from the atmospheric air, and forming an acid by combining with it.

The next observation which occurs, is—that if this acrid matter existed previously in the blood vessels, it must have been capable of uniting with the blood, without producing any apparent alteration of its texture, when examined out of the vessels.

But, although the bronchial vessels are extremely irritable, so that the minutest particle of extraneous matter may be sufficient to stimulate them; yet, as the vesicles in which they every where * terminate, *may be* endowed with a less degree of susceptibility than the larger branches, it is possible that a certain accumulation may be permitted, before the effusion becomes capable of exciting the action of the respiratory muscles to expel it. The *nature* of the effect, when exerted, is, however, precisely the same.

In the history of Asthma, it seems to have been admitted, that the effusion may produce irritation, “by its oppressive bulk † or acrid quality,

* Winslow's Anatomy, Vol. 2. Sect. 9. p. 105. 137.

Chefelden's Anatomy, p. 173.

† Practical Enquiry, p. 318.

“ or

“ or both,” and yet its exclusion from the blood vessels has been referred to a particular state of the arterial system, unconnected with either of these circumstances: for you have deliberately assigned *a morbid and * atonic state of the pulmonary capillaries, as the cause of the effusion of serum.*

Serum is undoubtedly expectorated, often, in considerable quantities, by persons who sustain the paroxysm of Asthma. It is discovered, often, in considerable quantities, in the bronchia of those who are cut off by the violence of the disease:—but, although the stimulus of quality, as well as quantity, is not denied, an *imaginary* state of the blood vessels has been resorted to, in order to account for the expulsion of the fluid from them, rather than it should be referred to either of the causes, which are acknowledged to influence the bronchial vessels, in the expectoration of it afterwards.

For these accounts of the relaxation and debility of the exhalent arteries of the lungs, we are indebted to those writers, who,

“ Nobly deserting common sense,

“ For metaphysic excellence,”

have thought it proper that they should *attempt to account* for every circumstance † in the history of

* Practical Enquiry, p. 157.

† “ The coldness of the lower limbs proceeds from THE DEFICIENCY OF ANIMAL HEAT; and the IRRITABILITY of the mind “ from the anxiety of the præcordia.”

Practical Enquiry, p. 235.

Asthma; and who did not remember that *all our Theory had better perish*, than that “one jot or one tittle” of it should rest on the unstable foundation of conjectural hypothesis.

It has been remarked, that every considerable alteration in the fluids, as well as in the solids of the body, is accompanied by correspondent symptoms. Now, it will not be denied that the state of the pulse varies, with every change in the quantity, if not in the quality of the blood, as well as of the rigidity or relaxation of the vessels which contain it. We have only, then, to resort to this faithful index, in order to discover what is the particular state of the vessels during Asthma, and what changes have taken place in the circulation.

Whenever the quantity of the circulating fluid be diminished, the beat of the pulse * will necessarily be smaller, in proportion to the measure of blood thrown out by the left ventricle: and whenever there be an increase of quantity, a fuller measure being expelled from the heart, will consequently occasion a proportionable increase of the sensation of fulness in the pulsations.

These varieties will obtain, whether the arteries, which contain the blood, be strong or weak, rigid or relaxed:—for the pulse may be hard and

* Fordyce,

small,

small, or soft and full, or hard and full, or soft and small, at the same time; for, as the fulness and smallness depend on the contained fluid, so the hardness and softness are influenced by the state of the containing vessels.

It is unnecessary for me to contrast the feel of fulness with that of contraction, an evident state of the arteries on particular occasions; I proceed, therefore, to mention other sensations, called strength and weakness, which are attributable, in most cases (though, perhaps, not in all) to the vessels; and increased and diminished celerity, which are controlled by the frequency of the contractions of the ventricle; and may, therefore, take place, when the arteries are either rigid, so as to give the sense of hardness, or relaxed, so as to occasion the feel of softness at the same time*.

The pulsation of the arteries may be increased in frequency by any muscular exertion which propels the blood with greater rapidity than usual to the heart. It may also be accelerated by other causes, when the muscles are not excited to action.

* This account of the pulse appears to be sufficient for the purpose intended in the present argument: but, as it is of great importance that the varieties of the circulation be minutely attended to; I should have been more particular in this part of my letter, if I did not entertain some intention of publishing a Tract, (lately composed, at the entreaty of a medical student, to be read as an inaugural dissertation, on taking a Degree in Physic) which contains a general history of the circulation, with all its phenomena.

In fever, while the body remains perfectly at rest, and the passions of the mind unagitated; the pulse sometimes increases to an hundred and fifty strokes in a minute, or even so as not to be counted. Fever, however, as I have before observed, by no means depends on this frequency. I have attended patients in Typhus Fever (such as I have endeavored to describe in a short Essay*, lately published) where the ordinary number of pulsations varied very little from the healthy standard; and, in the month of July last, the pulse of Mr. *Smith*, of *Jacknett*, near *Knowle*, in this county, remained exactly at 73 in a minute, for at least some hours every day, for several days during the continuance of a similar disease; and at the same time when the dark fur on the tongue with petechiæ were present.

In many other diseases, as well as fever, and in many of the passions of the mind, great increase of frequency in the pulse, is observable, without any exertion of the muscles.

The celerity of the circulation may, therefore, depend either on the heart having acquired an increase of susceptibility, or on the blood having gained an increase of the power of stimulating: for, the heart is not merely excited to contract on its contents, by the stimulus of distention; else in those persons who have the *foramen ovale* remaining open,

* Essay on the Nature and Treatment of a Putrid Malignant Fever, octavo, Rivingtons, London.

the circulation would not cease *so soon* as it is known to do, when the action of the lungs has been suspended.

It would be superfluous for me to enlarge upon the ingenious experiments, already so well known, which have illustrated the manner in which the oxygen received into the blood, from the atmospheric air, is conducive to the maintenance of that perpetual stimulus which excites the heart to contract on its contents.

The heart and arteries, may probably often differ in their relative * proportions of strength; and the heart, itself, may, under some circumstances, be *more* disposed to contract, that is, more susceptible of stimulus than at other times, like other living fibres of the body; and it may also become *less* capable of suffering irritation. It is sufficient, however, for me to consider it, at present, as influenced to contract, by the *quality* of the fluid passing through its cavities.

Now, it cannot be denied, that the circulation frequently becomes more rapid, without any increase of hardness or softness, of strength or weakness, which manifestly depend on the vessels; these sensations, therefore, not necessarily accompanying the increased celerity, evidently prove, that the latter depends on the increased frequency of the

* See Dr. Cullen's Treatise on the Materia Medica, Vol. 1.

contractions

contractions of the ventricle, excited by the peculiar property of the circulating fluid, and not occasioned by any alteration in the heart itself.

If it be admitted that the blood does stimulate the ventricle by its quality, and that that quality may allow of variation; it necessarily follows, that the ventricle may be stimulated to more frequent contractions, whenever the blood contains a greater proportion of irritating power.

How, then, can it be asserted, that *the heart is insensible to every stimulus but that of the blood, possessing its healthy * qualities?*

After this view of the subject, I do not think it worth while to trouble you with any remarks on Baron Haller's *assurance*, "that † the heart has very "few nerves;" nor on M. Behrends's opinion on the same subject.

* Practical Enquiry, p. 237.

† "Haller ASSURES us, that the substance of the heart has very "few nerves; and M. Behrends ALLOWS a still more inconsiderable "number, and those of TRIFLING magnitude."

Practical Enquiry, p. 237.

With regard to the LATTER opinion, as the work from which it was extracted is not referred to, in the "Practical Enquiry;" it may not be improper to remark, that it is only the sentiment of a STUDENT, delivered in an academical exercise at a foreign University:—See "Dissert. Inaug. Anatomico-Physiologica quâ demonstratur cor Nervis vis carere: additâ disquisitione de Vi Nervorum Arterias cingentium. "Auct. J. B. J. Behrends."

I come

I come, in the next place to, consider the state of the pulse during Asthma ; and its indications.

One condition of the pulse only, appears to have been noticed in the first part of your “ Practical Enquiry :”—it is the quickness*, unaccompanied by hardness, which prevails at the commencement of the paroxysm. I am not to enquire how it has happened, that the varieties of the circulation have been so little attended to : but shall endeavour to supply what appears to be a material deficiency, in the best manner I am able.

At the attack of a paroxysm of Asthma, a degree of fulness is commonly observable in the pulse, accompanied with some increase of celerity. This fulness is not always proportionate to the quantity of mucus to be *expectorated*; but, I believe, is always exactly regulated by the quantity of serous effusion.

The fulness, as well as the quickness, depends on the state of the blood itself, and is clearly unconnected with the condition of the arterial system.

The indication of the former cannot be misunderstood ; the latter evidently points out an increase of stimulating power.

* Practical Enquiry, p. 79.

When

When the living principle is unusually exhausted by muscular exertion, the blood is hurried with increasing frequency to the lungs, that it may receive a larger supply of oxygen: but when the muscular efforts cease, the blood no longer requiring such an extraordinary quantity, the circulation becomes more tranquil, and the pulse less frequent.

But, in Asthma, where no muscular exertions have taken place (I mean before the extravasation of serum has been effected) we find the pulse increased in frequency.

This frequency, then, must depend on some cause, different from the change produced in the blood by muscular motion, and the consequent increasing desire of a fresh supply of oxygen.

It has been said, that this symptom of an alteration in the property of the circulating fluid, is one of the appearances which precede the asthmatic paroxysm. It is not my intention, in this place, to enquire, *how long* it may have preceded it; but it is sufficient, for my present purpose, to distinguish it from any increase of frequency in the pulsations, which may arise *subsequent* to the exudation of serum into the lungs, and which may be occasioned by excessive action of the respiratory muscles.

I am

I am now to enquire what happens in the advanced stages of the disease.

“After some hours of distress,” in the paroxysm, when a deposit of the offensive matter has been made in the pulmonary vesicles, the frequency of the pulse diminishes, and it becomes less full, or, as you have termed it, “more * open.”

Is it not very evident, from this alteration, that the cause of the preceding symptoms must have been, the presence of some stimulating material in the blood vessels, which has now been, at least partially, removed?

It is admitted, that the heart does not contract from the stimulus of distension only; but that its pulsations are excited by the properties of the blood. The mere increase of serum, therefore, which may have occasioned the *fulness* of the pulse, is not sufficient to account for its celerity. No state of the arteries, to which we have yet adverted, will at all illustrate this phenomenon. Neither hardness nor softness, neither strength nor weakness, has any thing to do with it. It is, however, still possible to explain it, and some assistance may be derived, from considering the symptoms by which it is accompanied.

* Practical Enquiry, p. 73.

“ A tingling * and heat,” in various parts of the body, is one of the most constant symptoms which accompany this disease; but “ generally † declining as the agony of respiration increases.”

This sensation, which I have already noticed, has been referred (but very unsatisfactorily) to a multiplicity of causes, and among them to “ acrimony ‡” which you have informed us, “ is || to be discharged on the skin,” but without explaining in what state, or in what vessels it exists; from what causes, or by what process it is formed. From reason, however; from the state of the pulse; from the analogy of effects, which we are enabled to trace to the use of certain substances as part of our food; from the occurrence of the same symptom in other diseases; and from its declining, at the same time when the frequency of the pulse diminishes, namely, when the “ agony of respiration increases” (when the offensive matter has been expelled) I infer that this “ tingling sensation and feel of heat,” is occasioned *by the acrid quality which the blood has acquired:* and I assume it, as a collateral proof of what has been before advanced, respecting the cause of the quickness of the pulse in Asthma.

They who suppose that the sensation, above-mentioned, is occasioned by “ the ¶ difficulty with

* Practical Enquiry, p. 77.

† Ibid. 214.

‡ Ibid.

|| Ibid.

¶ Practical Enquiry, p. 215.

“ which

“ which the venous blood is returned to the heart,” must have forgotten that it *precedes* that obstruction in the circulation ; and as you, yourself, acknowledge, declines when the agony of respiration comes on.

But although these convincing proofs may be adduced, of the blood having acquired an increased power of stimulating the heart and arteries ; there are no symptoms which, in the uncomplicated species of Asthma, afford any reasonable pretext for believing that the blood vessels, themselves, have suffered any considerable alteration from their usual state.

In anasarctous habits the vascular system is universally relaxed ; the pulse is consequently weak, and often soft. In those habits, also, of long confirmed Asthma, which are governed by the constant prevalence of dyspepsy, the vessels become relaxed, and the pulse is weaker, than in younger subjects, and in the early attacks of the disease.

In studying the natural history of any morbid affection, we should examine it in its simplest and least complicated form ; for, from thence alone, can we derive the knowledge of its causes.

In the first attack of Asthma, the patient not being of a constitution greatly relaxed, the pulse is *full*, without being remarkably weak, and *quick*, without much hardness. Neither of these sensations indicate that extreme relaxation of the arteries,

which has been adduced, by some authors to account for the extravasation of serum.

It is true, that a very relaxed state of the vessels does sometimes prevail, when Asthma takes place; but it is certainly not requisite for the production of the disease: because it frequently arises, and goes through its progress, without any such relaxation being at all discoverable.

If the effusion of serum really depended on a torpor of the capillary vessels, such a state must necessarily be marked by a weak pulse: and the system partaking of the same disposition,—anasarca would as necessarily be a constant companion of Asthma; but dyspepsy is not *necessarily accompanied*, * though often followed, by watery blood.

In order to account for the exclusion of the offending matter, we have only to resort to analogy. Every organ of the body is exactly fitted to act upon, and to be acted upon by, peculiar modifications of matter. Thus, the liver is adapted to the secretion of bile *from* the blood; and is so organized as to suffer an excitement, or a stimulus, to perform that secretion, by the qualities *of* the blood. These qualities, being capable of great variation, the degrees of secretion must differ in proportion: and if any extraneous matter be, by any means, forced into the vessels destined for secretion, such

* Practical Enquiry, p. 240.

matter will be expelled by an effort of nature, which is always aroused to rid itself of noxious or oppressive matter.

In the less complicated organs, as in the urinary bladder, designed, merely, as a receptacle for the aqueous part of the blood, separated in the kidneys; extraneous bodies (such as calculi) are no sooner formed, than the containing parts are stimulated to expel them. Whether the effort produced, be sufficient for that purpose or not, makes no part of the present enquiry.

Urine may be accumulated in a very considerable quantity, before it excites the bladder to contract, and expel it, by the stimulus of distension only; but when the contained fluid has acquired an augmentation of its stimulating property, the bladder may contract, on a very small quantity, with great violence. In like manner, the gall bladder, the stomach, the intestines, and, in short, every containing part, is irritated to endeavour at the expulsion of that which offends it, or has a tendency to interrupt its * functions.

The effort of the heart and arteries is thus excited, by the acrimony of the serum circulating through them in Asthma; and their extremities, partaking of the irritation, open their orifices, and exclude the offending matter.

* Practical Enquiry, p. 16.

The circulation is then restored to a more natural state, and remains so, until an increase of acrimony again stimulates the vessels, and reinduces the paroxysm.

This accounts for the diminution of frequency in the pulse, when the extravasation of serum has been accomplished; and explains the phenomenon of irregular periodical returns of the disease, without allowing to the capillaries “a power * of contracting, independent of the trunks from which they proceed;” or ascribing to them “a debility† which does not, proportionably, correspond with the tone of the larger vessels.” It points out the true cause of the asthmatic paroxysm, in those cases, in which *you* have attributed it to the injudicious ‡ use of the lancet: or to acrimony, which, in another place, you have said, is “always present *in the atmosphere*, and ready to be || inspired.” It destroys the ridiculous idea of Asthma, having been induced by the use of mercurial preparations**, and also satisfactorily explains those symptoms which have often been referred to various, and, indeed, opposite †† causes; it supercedes the neces-

* Practical Enquiry, p. 158.

† Ibid.

‡ Ibid. 296.

|| Ibid. 318.

** Chamberlaine's Treatise, &c. p. 10.

†† The notion of sympathy, which Heister, not improperly, termed, an ENTHUSIASTIC DOCTRINE*, has also furnished the

* Heister's Surgery, Part 2d. Sect. 1. C. 2.

sity of Dr. Girtanner's hypothesis respecting the insensibility of the heart, which he has *luckily* not attempted to prove, because he could *not have proved* it, if he had attempted it: and it elucidates the causes of an intermitting pulse, without the doctrine of "*the heart sensibly * feeling the want of stimulus*;" which reminds me of Mr. John Hunter's observation, who was accustomed to speak of *the stimulus of death*: an expression, perhaps, the least determinate of any of which he could have made use; for it must mean a sensation of which no man can form the least idea, *who has not felt it*, and which no man can describe, *who has*.

But if it could be conceived possible, that a partial atony of the capillary vessels might prevail;

shadow of a cause of Asthma; which, though ONLY A SHADOW, has been eagerly caught at, by the older writers. You remark, that "WILLIS speaks of Asthma from SYMPATHY, arising from A STONE in the gall bladder." (page 331.) The case alluded to, was, I suppose, that which is introduced in his "Pathology of the Brain and Nerves" of a "Butcher at Walsall," (which, by the bye, is given at SECOND, or perhaps, third hand, for WILLIS observes, that "it was communicated" to him, "by the learned Dr. WALTER NEEDHAM," who happened to reside at the CHARTER-HOUSE, in LONDON :) about whose symptoms we are entirely in the dark, though not more than WILLIS, himself, who has not attempted to describe them; and, therefore, his idea of the man's disease arising in consequence of biliary obstruction, or to translate his words more literally, of "the gall bladder containing many stones," might have been deemed capricious and unsatisfactory; even if NO BETTER theory could be suggested.

* Practical Enquiry, p. 235.

the

the exhalents of the lungs only, being affected, it would then become a question, how is the tone of these vessels restored?

After a certain period, we find, that the paroxysm of Asthma terminates, spontaneously, by the expectoration of a larger or smaller quantity of mucus; but no fresh deposit is made for some time afterwards. If the extravasation took place in consequence of a relaxed state of the arterial extremities, become incapable of contracting with their accustomed energy; these vessels would continue to permit the exit of their contents so long as any serum remained of a thin consistence, or until they had regained their contractile power.

When the disease is not left entirely to itself, the remedies exhibited during the paroxysm, though of various kinds, and very dissimilar in their modes of operation, are in no wise of such a class as to afford the least probability that they have, at any time, contributed to restore the energy of the exhalent arteries, or to invigorate the system.

Hence it appears, that the effusion of serum, in Asthma, is *not* necessarily to be referred to a preternatural relaxation of the blood vessels: notwithstanding all the arguments which have been wasted in support of that hypothesis.

I have already offered, what I conceive will be admitted as sufficient proof, of the existence of an
increase

increase of stimulating power, in the blood of persons afflicted with Asthma.

The next step is, to ascertain in what manner such an alteration of quality be acquired: and this enquiry I shall endeavour to pursue by the same plain method of argument, which, I trust, has been successfully employed in the preceding pages; not forgetting Sir Isaac Newton's remark, which you have quoted, "*Frustra fit per pleura, quod fieri potest per pauciora.*"

The appearance of blood, when examined out of the body, in, apparently, the same circumstances, is found to vary very considerably: for the atmospheric air, being capable of effecting a change in its colour, and the temperature of the surrounding medium, either accelerating or retarding its coagulation, and the shape of the vessel into which it is received, and the size of the stream by which it flows, having each its respective share of influence upon it; there is a considerable degree of difficulty in conducting experiments on this important fluid, with sufficient accuracy.

The component parts of the blood have, however, at length, been ascertained. The first, and most obvious division is into red particles*, coagu-

* FORDYCE.

See, also, the works of BOYLE, SENAC, HALLER, JURIN, Father de la TORRE, HUNTER, but particularly HEWSON's Experiments.

M

lable

lable lymph, and serum. It is unnecessary, on the present occasion, to pursue the examination beyond this view of the matter: because, if it be admitted that either of these parts, being increased or diminished in quantity, must create an alteration in the whole mass, it will be readily conceived that each of them, being in itself a compound, will necessarily vary, in proportion to any change which may have been effected in the parts of which it is compounded.

In some diseases, the texture of the blood is broken down to an infinite minuteness; in others, the whole mass is rendered glutinous, by the deficiency of serum: in others, a vast quantity of superfluous water is circulated in the vessels, in an uncombined state.

By the want of one of the properties, usually superadded to the blood, by the atmospheric air in the lungs, its colour becomes altered. In such cases it may be considered, either as merely deficient of a necessary component part, which ought to have been communicated to it; or as having retained some excrementitious particles, which that process would have removed.

Besides these disorders and alterations in the blood, it is liable to be mixed with various fluids, secreted by different organs of the body. For example, in jaundice, a very copious secretion of
bile

bile takes place, which is conveyed into the circulation by the absorbents.

Bile is a fluid, whose colour, taste, and smell, are so well known, that no person, who has ever seen the blood of a patient in jaundice, can possibly mistake its appearances, or refer them to any other than their true cause.

This single fact is sufficient to prove, that *the absorbents may convey into the blood vessels, a fluid capable of effecting an alteration in the taste, colour, and qualities of the serum.*

Experience, also, convinces us, that the finer parts of Cantharides, applied to the skin, are absorbed into the blood vessels, and passing from thence to the bladder, irritate it by *a constant stimulus*, to attempt the evacuation of its contents: an effect never produced by the urine in a healthy state,

The odorous parts of asparagus, and of garlick, are also found to combine very readily with the blood, and to pass off with the urine.

Almost all the authors, who have treated of the doctrine of Asthma, have agreed in assigning to it, dyspepsy as one of the predisponent causes, which “induces that *state of the body* in which the “former disease takes place.”

In this, you, Sir, have concurred with the great and the learned, the ancient and the modern writers, to whose works you have so frequently referred; but, like them, have gone no further, nor informed your readers what that state of the body is: nor *how* * dyspepsy does produce it. "Wherever," you remark, "dyspepsia prevails, there shall we find a *fruitful* opportunity of exciting the paroxysm of Asthma; but this morbid debility of the stomach *must probably concur with accidental causes*, before the disease appears†."

The laborious industry and ingenious experiments of the Abbé Spallanzani, Dr. George Fordyce, and others, have proved in what manner chyle for the support of animal life, by its conversion into blood, is separated from the food by the process of digestion: and it has been shewn that if any substances, capable of an alteration, in any degree of temperature, not greater than that of the body, should be retained in the stomach, without being digested; or, in other words, if the stomach, by

* It is really astonishing that authors should have approached so near to the truth, without perceiving it. Dyspepsy has been observed to ACCOMPANY Asthma, and yet the manner in which the latter was influenced by it, has been totally overlooked; and even the most obvious mode, by which the remedies employed, were productive of advantage, entirely neglected. Some of the mischiefs of Indigestion were noticed, at a very early period; even Hippocrates was aware of them, and Galen attributed * ALL diseases to that source.

* Galen, lib. i. de cibo.

† Practical Enquiry, p. 260.

disease,

disease, or by a deficiency of any of the materials necessary to be employed in digestion, should have become incapable of performing that process, even for the shortest space of time, while substances which might otherwise be converted into chyle, remain in it; a fermentation takes place, by which an acid is *necessarily* disengaged:—"the formation
 " of acid * being always produced by the digestion
 " not going on perfectly."

Acid, therefore, may be formed in very considerable quantity; and it may be ejected through the intestines, in consequence of the irritation which is produced by it, or by the process which takes place at its formation. But it may also be retained in the stomach and intestines, till the absorbents, being stimulated, convey it with the chyle into the blood vessels.

Among all those circumstances which are said to produce a predisposition to Asthma, Dyspepsy stands foremost. "Dyspepsia is a condition of the
 " habit which will be found *always* to have preceded the periodic Asthma†." Permanent Dyspepsy is that state in which a great quantity of acid is extricated from the undigested aliment;—the acid thus formed, I consider, therefore, to be the *real cause* of Asthma; and it is certainly no ob-

* Fordyce on the Digestion of Food.
 See also Spallanzani on Digestion.

† Practical Enquiry, p. 217.

jection to this opinion, that it may be urged Dyspepsy does not *always* produce that disease.

It is undoubtedly necessary that the acid be formed in considerable quantity, and that it acquire a certain degree of bulk (if I may be allowed the expression) before its stimulating power can be exerted, with success, in the blood vessels. I have shewn that the pulse is not without an indication of the stimulus of distension, as well as of quality; and my observations are supported by your own remark, that quality and quantity may combine their efforts.

It has been observed that the diabetes, which frequently accompanies Asthma, “ does * not, in fact, apparently alleviate the labour of respiration; it seems, on the contrary, to *precede*, or *accompany*, the effusion into the vesiculæ, instead of following it:”—it is, therefore, undoubtedly dependent on the same cause which produces the effusion into the lungs,—the irritation of acid serum in the blood vessels.

If, then, a definition of Asthma be required, I have no objection to call it, *an excessive contraction of the respiratory muscles, excited by the irritation of acid serum effused from the pulmonary vessels into the vesiculæ and bronchia.*

* Practical Enquiry, p. 228.

It may, perhaps, be asked, How comes it to pass that the use of *vinegar* in the paroxysm of Asthma is productive of so much benefit, if the disease really depend on the predominance of acidity?

To which I answer. This remedy is confined to the paroxysm; in which it is prescribed as an expectorant,* and, perhaps, a diuretic, and no practitioner would think of employing it, *afterwards*, as a tonic; or, for the purpose of restoring those powers of the system, whose healthy action is to prevent the recurrence of Asthma.

Thus, emetics are advantageously prescribed, for the purpose of removing offensive matter, such as indigestible or fermenting aliment, from the stomach; but no one would think of using them to

* The ancients seem to have considered ALL acids as similar in their mode of action:—thus the muriatic acid, or, as it was formerly called, “spirit of sea salt,” was greatly praised by Van-Helmont “EST NAMQUE ACIDISSIMUS” (said he) “nec sibi par habet remedium, “extinguendis ardoribus urinæ etiam præstanti in vesica calculo.”

Vinegar is also a very old remedy, having been long celebrated as a powerful diuretic and anti-emetic. It was strongly recommended by Dr. BAYNARD, Dr. BENTLEY, Mr. KEMP*, Dr. COLBATCH†, and other practitioners of the last century, who only improved the compliments bestowed on it, by CELSUS, DIOSCORIDES and MINDERERUS:—it was prescribed by BOERHAAVE as a sudorific, and has since been noticed by DARWIN as AN ABSORBENT, in terms nearly similar to those adopted in the “Practical Enquiry.”

* Author of “a Treatise on the Pestilence.”

† Author of “Physico-medical Essays: Novum lumen Chirurgiæ, &c.
invigorate

invigorate the body, or to increase the permanent strength and tone of the stomach, afterwards.

Throughout the whole course of this investigation, I have hitherto considered the Theory which I have endeavoured to establish, as destitute of any other proof than its own reasonableness, and the evident relation between cause and effect: but I have not yet put forth *half my strength*, and as it may afford some degree of satisfaction to those who are desirous of seeing just theory founded on its most certain basis, I think it right to add, that the opinion which I have ventured to deliver, is supported by a series of experiments, conducted with equal accuracy and judgment, by Dr. Bache, Physician at Birmingham. This gentleman, whose diligent and scientific attention to every subject of practical importance in medicine is only equalled by his candour and integrity; was induced to investigate the different secretions of an asthmatic patient, so long ago as about the year 1784.

In this case, which is very distinctly related, “the * saliva was remarkably frothy, and the expectoration extremely viscid. The surface of the urine (after it had stood a few hours) seemed as if covered with dust,” which, being collected and dried, was “subjected to examination by the microscope,” and proved to be saline crystals,” but a

* Medical and Physical Journal, Vol. 2, p. 140.

sufficient

sufficient quantity could not be obtained, for any chemical experiment.

Lime-water was precipitated by this urine with greater celerity, than by the urine of a person in health.

“ The * matter perspired, smelt *sour*; and when
 “ pieces of bibulous paper were previously stained
 “ by a solution of litmus, and then applied to
 “ various parts of the body, the exudation from
 “ each, produced the effect of a weak acid upon
 “ the colour; and when the paper was made dry,
 “ and the edge of it applied to the flame of a
 “ candle,” it was “ found to be a weak touch pa-
 “ per; but when immersed in a small glass of water,
 “ in which a grain or two of vegetable alkali had
 “ been previously dissolved, and dried a second
 “ time, its property as a touch paper was greatly
 “ augmented.”

In short, the result of the Doctor's enquiry convinced him, that “ *an † acid pervaded the whole of*
 “ *the circulating system:*” and suggested the mode of cure, which was adopted with complete success.

It is with equal respect and gratitude that I embrace this opportunity of acknowledging my

* Medical and Physical Journal, Vol. 2. p. 141.

† Ibid.

obligations to Dr. Bache, for the alacrity with which he politely communicated to me the particulars of the experiments, above alluded to, and the very liberal and candid manner in which he favoured me with many valuable remarks on the subject.

The interests of science, are closely connected with the ingenuoufness and liberality of those who walk in her paths; and I am persuaded that every one of my readers will concur with me, in the opinion, that he who imparts to his friends, without reserve, and communicates to the world, without ostentation, the result of judiciously directed studies, and attentive experience, adds lustre to his character as a Man, by his conduct as a Practitioner.

To proceed;—some of the secretions have been examined by others, from different motives, and with different views. The copious evacuation of urine which frequently takes place, at the time when the ferous extravasation, into the vesicles commences, has occasioned that fluid to be more particularly attended to. I do not know whether its *acidity* has been often observed, nor (if detected) that it has always been remembered: but, although it is possible that the urine may not *invariably* partake, in so great a degree, of the prevailing acidity; this, by no means, disproves the general theory which I am anxious to establish;—but only so far anxious, as it will lead to a more certain mode of practice;

practice; and encourage the faculty to regard what has hitherto been considered, only an accidental concomitant of the disease, as the *primum mobile*. You, Sir, have examined this secretion, and, certainly, do not mention its having acquired the property I have described; but although I am far from doubting the accuracy of your statement, and am not inclined to dispute your *taste**, I humbly conceive that such an alteration might have escaped detection, in the mode you observed; particularly as the flavour of urine cannot be supposed very familiar to the palate.

If you ask in what manner acids are productive of irritation, I will candidly confess, that I am entirely ignorant of it.—It appears to me, sufficient, that I have proved the formation of acid, its con-

• “If the Urine is copious and pale, it is, I believe, never “SWEET.” Who ever supposed that it was?

“I have TASTED the water of an Asthmatic more than once, “when the circumstance of quantity and appearance strongly solicited “my curiosity, but I always found it WEAK, SALINE, and of no “SACHARINE TASTE whatever.” Practical Enquiry, p. 248.

The judgment which some have pretended to form of the nature and symptoms of diseases, from an examination of Urine, by THE EYE AND THE PALATE, has been often exposed to deserved ridicule; and as no real dependence can ever be placed on SUCH observations, it is unfortunate, as well as surprising, that practitioners have not been deterred from recording them, by the example of a late celebrated Physician at BIRMINGHAM, who is said to have gravely decided on a state of pregnancy, from the inspection of Urine, which, in reality, belonged to a little girl. It is, I suppose, unnecessary to add, that the learned Doctor immediately received his fee of dismissal: and that the respect and confidence of his patient was not GREATLY INCREASED by this proof of physical sagacity.

veyance into the blood vessels, and the subsequent effects which are produced by it. I pretend to no further knowledge of the manner in which these events arise: and it would be far more conducive to the true interests of humanity, and the advancement of science, if men would more frequently content themselves with attentively observing the appearances of effects*, and faithfully recording them, rather than seek after Fame in the wide fields of Theory and Hypothesis†. We subject ourselves to the reproaches of our contemporaries and the ridicule of posterity, by vain efforts to explain what exceeds our comprehension; and often become, like the blind man (mentioned by Mr. *Locke*‡,) who attempting to describe a scarlet colour, said that it resembled *the sound of a trumpet*.

In almost every disease, there are symptoms for which human sagacity has not yet been able satisfactorily to account. Let us exert our unremitting endeavours to arrive at a more correct knowledge of them; but be always more ready to confess our ignorance, than to disguise it by affectation; for “the ¶ human mind has its horizon as well as the “organs of vision. The boundaries of nature are “extended beyond our reach; and as the laws of

* Sydenham.

“† It becomes the Physician to repress those wild sallies of imagination which lead to ridiculous hypotheses, and to admit only such “facts as are established by accurate observation.”

Fourcroy's Elements of Chemistry, Vol. 1.

‡ Locke on the Human Understanding.

¶ Bache on Electricity.

“matter

“ matter are too much divaricated for its complete
 “ investigation, it becomes us to remain satisfied,
 “ if assiduity, and the exertion of our mental powers,
 “ can bring us within the sphere of probability,
 “ where mathematical demonstration, or *positive*
 “ *proof*, cannot be obtained.”

It is possible that *the nature* of the *acid*, itself, which I consider as the *cause* of Asthma, may differ in particular cases: nor do I altogether despair of being able to pursue this enquiry with considerable advantage, if many opportunities of attending to the disease should be afforded me.

It remains for me to say a few words on the subject of medical treatment.

There are so few medicines, which, being considered *a priori*, would appear capable of relieving diseases, that it is not surprising that many of the most valuable should have been the discovery of accident: and the almost insuperable difficulty of explaining the manner in which they produce their effects, has occasioned Practitioners, in general, to continue their use, without much reflection.

Ingenious men, excited by curiosity, and encouraged by the activity of their imagination, have sometimes indulged themselves in speculating on this intricate subject; but the degree of certainty, to which the utmost efforts of human intellect have been able to attain, has hitherto been productive of,
 comparatively,

comparatively, few improvements in the practice of medicine.

GOD has been pleased, in his wisdom, to establish a relationship between the affinities of bodies, by a series of dependencies, which, springing one out of another, constitute an universal chain; of whose links, only a few are perceptible by the limited powers of his creatures. These links then, are called causes and effects, but our imperfect discernment, only grasps at some of them, continues its researches through a few more, then loses itself in visionary hypothesis or vague conjecture; jumps, perhaps, again, upon a different part of the series, and ultimately acquires just sufficient knowledge of the whole, to admire the wonderful intelligence and perfection of its primeval cause.

But, notwithstanding this state of imperfection, the mind is endowed with a constant activity, never resting satisfied with its present acquirements, but delighting in pursuing its speculations, although so unfortunately prone to wander from the truth.

It is only by attending to the experience that one event * springs out of another, that we establish the idea of the mutual relation between cause and effect: and the mind being capable of supposing an existence, *independent* of any cause, is

* Fordyce.

apt to lose sight of the *dependence* which other objects have on one another; and to substitute an imaginary connexion instead of that which cannot, perhaps, be traced without great difficulty, and the most attentive investigation.

Hence have arisen that vast variety of fanciful theories, which, being founded on error, are left to float at random, without stability or advantage. In physic, as in other sciences, "error and obstinacy, the pride of system, and the influence of authority, have conspired in too many instances, to obscure the plainest indications of nature*:" and we ought to think ourselves happy, if, by patient industry, we can obtain the possession of two or three links, at no great distance from each other, in the chain of causes and events; for, although the part which connected them, may, perhaps, be irrecoverably lost, the mechanism of those links which we have discovered, will, sometimes, lead to a tolerably conclusive idea of its shape and texture.

Thus we know that a grain of wheat, being put into the ground, produces a plant, which, when it has arrived at maturity, exactly resembles that from which the seed was taken:—repeated experience convinces us of the fact, although no human study nor exertions have been able to discover *how* the germination of the plant, or the evolution of its parts is effected.

* Practical Enquiry, p. 322.

Proofs have been adduced in the preceding pages, that a great quantity of acid being extricated in the fermentation of undigested aliment, is conveyed into the blood vessels, and stimulates them to expel it: but I know no more *how* or *why* it is the *nature of this acid* to produce such an effect, than I know *why* grain buried in the earth should produce a plant.

In the very nature of things it is necessary that matter, should, at its original formation, be endowed with *distinct properties*; and it is sufficient *for me* to believe, that the all powerful Creator has distributed these properties with infinite wisdom: and this belief results from analogy, from contemplating that beautiful harmony of arrangement, and excellence of contrivance, manifested in those things which my senses *can* comprehend.

Leaving, therefore, all metaphysical reasoning on the *modus operandi*, I proceed to notice such medicines as have been generally resorted to, for the removal of the paroxysm of Asthma, or to prevent its recurrence.

If any proof be required of the erroneous notions, formerly entertained, respecting this disease, it may be obtained from a review of the medicines which have been employed for its removal.

In the number of those prescribed for asthmatic patients, many, very contrary in their tendency and effects, have, at different periods, obtained the favourable opinion of practitioners; who have exerted no inconsiderable degree of ingenuity, in varying their different forms * and combinations.

The ancients, supposing that *a defluxion from the head* was the cause of Asthma, maintained the necessity of using evacuants. The moderns, as if to complete the perfection of former error, have sometimes † employed them at the commencement of the paroxysm: but, as a mere removal of undigested aliment, is the only real benefit which they are capable of affording; cathartics should only be used as assistants in the general intention of restoring the healthy functions of the organs of digestion.

Emetics, having some tendency to promote expectoration, and likewise diminishing the quantity of acid formed in the stomach of an Asthmatic, were found serviceable; although they were undoubtedly not employed with the latter intention, either by Baglivi, Etmuller, or Sir John Floyer: but every violent effort of the system, at the beginning of the paroxysm, must evidently be injurious.

* Withering.

† Practical Enquiry, p. 360.

Long before the extravasated serum was considered to be the exciting cause of the convulsive efforts of the muscles of respiration, *expectorants** had been tried, and their effects found highly advantageous. They have been constantly and deservedly retained, from their first introduction into practice, to the present time.

Beneficial effects have been ascribed to blisters, but as the mode by which they produced them, was not at all understood; it has likewise been denied † that they were the means of relief.

The prevalence of acid irritating the blood vessels being proved, the use of blisters is at once explained, without supposing them capable of lessening the distension ‡ of the stomach.

As to their promoting || expectoration, which must depend on their excitement of muscular action; it would be just as reasonable to suppose that

* The bitter Vetch and Nasturtium were recommended as expectorants in diseases of the breast and lungs, at a very early period. The former is mentioned by PLINY, as having cured AUGUSTUS CÆSAR of a disease which was supposed to have arisen from viscid phlegm in the lungs. Even Vinegar of squills is recorded in one of the books which bear the name of GALEN.

See Parkinson's *Theatrum Botanicum*, p. 1079, London Edition, 1640.

† Practical Enquiry, p. 374.

‡ Ibid. 375.

|| Ibid.

the speed of a horse would be increased, or the strength of a gold-beater's arms, by applying a blister to his back.

It is unnecessary to expatiate on the inutility of fumigations of all kinds ; nor do I think it worth while to combat the *visionary* idea of *aerial* medicines being capable of subduing the violence of the paroxysm, *before* the ferous effusion has been expectorated.

After commenting upon the use of Vinegar in the paroxysm, you, Sir, have been pleased to express "great hopes of nitre in combination" * with that acid. As I do not very well understand the rationale of such a prescription, I will only observe, that this mixture, having been on record † for five and twenty hundred years, it is to be lamented that no determinate ideas of its efficacy have been hitherto established.

I hope it will not be expected that I should be particular on the effects of two remedies so directly opposite, as hot ‡ and cold bathing. The latter, as

* Practical Enquiry, p. 384.

† Bibl. Sacr. An. A. C. circa 700.

‡ "Not contented with probability, R. B." (Dr. BREE) "went into the hot bath, both in the intermission and the paroxysm. The respiratory distress was much aggravated," &c.

"In the paroxysm R. B. went into a bath of forty-six degrees. The great abstraction of heat was evidently injurious, and it was some hours before the impression was so far overcome, as to take away fears of the consequence." Practical Enquiry, p. 389.

This might not unaptly be termed ARGUMENTUM AD HOMINEM!

a tonic, requires no new panegyric:—the former appears so directly repugnant to all the reasoning which I have been able to apply to the subject, that I have never thought of directing it.

When the theory of spasm was *fashionable*, those practitioners who embraced it (and a vast number of pupils who supposed it necessary, that they who had been brought up at the feet of Dr. *Cullen*, should defend *all his whimsies*, did embrace it) thought themselves suddenly in possession of the philosopher's stone. Every disease was referred to it; all former doctrines were sacrificed at the altar of the idol, and its influence was extended over every medicine prescribed by its devotees.

Antispasmodics were tried in Asthma, and tried in vain. Zinc*, castor, musk, camphor, opium, and, in short, every preparation, with which the ingenuity of modern pharmacopolists has furnished us, have been repeated, and unsuccessfully repeated. Apo-

* Zinc scarcely deserves to be enrolled in this list, notwithstanding "the opinion of Gaubius*," for although it has been recommended in Epilepsy, and employed, (it is said) successfully in † Germany, though unsuccessfully in ‡ Great Britain; it is not very clear that it possesses any antispasmodic quality whatever. If it does, it is scarcely necessary, now, to observe, that it is not likely to be productive of benefit in Asthma.

* Practical Enquiry, p. 368.

† Fourcroy's Chemistry, Vol. 2. p. 391.

‡ Pharmacopœia Col. Reg. Med. Lond. [English Edition] p. 187.

plectic

plectic stupor*, the dreadful foe of rational existence, has been boldly dared, with obstinate and reiterated

* "In the access of a paroxysm R. B. took FOUR grains of solid opium, which produced, nearly, an apoplectic stupor for two days; a countenance more turgid than usual, and intense head ache attended. The paroxysm shewed itself four hours earlier, than usual, the next day, and TWO grains more were taken when it was perceived to commence:—anxiety encreased to an alarming degree as the stupor became something less. Loose motions succeeded, and a general sweat. A medical friend became alarmed." &c. Blisters were applied; Vinegar, and Pepper, and Mustard, and Coffee were given, and the patient was at last brought back to a state more usual in former paroxysms, but with every care the exacerbations were no fewer than "nine" before the end of the fit. But, "notwithstanding the bad success of this experiment. Opium gr, ij was used after an active vomit, and bad consequences still ensued:" so that, to use Dr. BREE's words on another occasion, "this gigantic remedy was neither productive of gigantic fame nor estimable reputation." p. 372.

The above reminds me of a still more conclusive experiment of the late eccentric Dr. STARK; and as his opinion has been resorted to, in the "Practical Enquiry" (page 175) this will, I trust, be a sufficient apology for introducing the following anecdote.

Dr. STARK * was employed several months in making experiments on his own frame, of the effects of different sorts of food, confining himself to a single kind for several days. He subsisted for ten days, on honey and bread, and a pudding composed of flour, water, and honey, by which a diarrhoea was produced, for which he ate a large quantity of Cheshire cheese daily, without using any other food or medicine. The diarrhoea was completely stopped. He then caused himself to be blooded; took a mixture of Tartarized Antimony and Rochelle Salts, which reinduced the diarrhoea,—with great debility, and anxiety about the præcordia: he became delirious, and died, at the age of twenty-nine, a Martyr to his own imprudence, and an Example to all future EXPERIMENTALISTS.

Dr. STARK has frequently been applauded for his industry and perseverance, but when we reflect on their fatal consequence, we shall be compelled to admit, that although he had ZEAL, it was "not according to KNOWLEDGE."

* See Monthly Review for April, 1790.

temerity.

temerity. Large and repeated doses of opium have been taken, in opposition to just reasoning, and in defiance of distressful experience.

Of stimulants I have nothing to say; and shall close my remarks on the medicines in common use, with observing, that a *proper idea* of the nature and cause of Asthma, being once established in the minds of practitioners,—the indications for its removal and prevention will become extremely obvious.

First, to remove the instrument of offence: and secondly, to prevent its regeneration.

The first, comprises the remedies which are required in the paroxysm.

The second, those which being used after the termination of the paroxysm, have a tendency to prevent its recurrence.

Expectorants may be advantageously employed, to remove the serous effusion, oppressing the vesiculæ and bronchia: and as the blood partakes of the cause of offence; whenever considerable fulness of the vessels be indicated by the pulse, at *the commencement* of a paroxysm, blood letting may be directed with a fair prospect of relief; and blisters may be successfully applied.

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When the violence of the paroxysm has been subdued; whatever has a disposition to correct acidity, and to restore the tone of the stomach, and other organs of digestion, is expressly indicated.

The benefit which has been derived from the use of *chalk*, during the prevalence of acidity, is too obvious to be overlooked entirely, or to demand my particular notice. But in the character of an *absorbent* only, can the advantages of chalk to asthmatic patients, be rationally accounted for: and your *conclusion* "that the capillary orifices of the stomach demand some appropriate astringent to excite their contractions," affords no real elucidation of the manner in which the use of *chalk* produces benefit. Quincy, * indeed, speaks very positively of the great astringency of chalk, but his authority is opposed by that of the indefatigable Dr. Lewis, † as well as many other respectable writers on the *Materia Medica*.

Bitters, Steel, and all that class of medicines which has been denominated corroborants, may be advantageously resorted to.

Chalybeate waters, which have a tendency to obviate costiveness, have been long esteemed as highly beneficial.

* Quincy's *Pharmacopœia officinalis*, p. 108.

The same author talks of acescent vegetables "chilling the juices, and occasioning them to run into corruption and putrefaction" !

† Lewis's *Dispensatory*, p. 130, art. *Creta*.

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Every habit of life, which, in any degree, affects the state of the digestive organs, should be carefully attended to.

The mere influence of habit, or sympathy, can only dispose a person to be more readily affected by disease: but habitual modes of life which tend to diminish the tone of the solids, should be avoided: and although it may seldom be practicable (if it were thought ever so highly advantageous) for an asthmatic to change his residence and alter his mode of life, and the nature of his pursuits entirely; a relaxation from the fatigue of serious study, and the dyspeptic provocation of sedentary employments should, if possible, be insisted upon.

Exercise on horseback is of singular advantage; and a careful attention to the species of * clothing worn by dyspeptic patients, is of great consequence.

In every particular case, the fit and the appropriate, must be left to the judgment of the practi-

* "The perspiration of the human body, being absorbed by a covering of flannel, is immediately distributed through the whole thickness of that substance, and by these means exposed, by a very large surface, to be carried off by the atmosphere; and the loss of this watery vapour, which the flannel sustains on the one side, by evaporation, being immediately restored from the other, in consequence of the strong attraction between the flannel and this vapour, the pores of the skin are disincumbered, and are continually surrounded by a dry, warm and salubrious atmosphere."

Sir Benjamin Thompson's (Count Rumford) "Experiments" inserted in the "Repertory of Arts," No. xxii, p. 251.

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tioner, and after thus explaining the *general intentions*, it would be not only useless, but impertinent, to attempt entering into a more minute view of the remedies which ought to be employed; for, notwithstanding whatever has been, or may be advanced, by the unlearned or the *credulous*, respecting *Specifics*; in medicine there is no such thing * as an universal law.

That modification of the disease which was formerly denominated dry or nervous Asthma; but has since been distinguished by the term *convulsive*; (and which consists of periodical returns of paroxysms, in which the action of the respiratory muscles, is, perhaps, equally violent, as when they contract, in consequence of a large quantity of mucus to be expelled from the lungs;) is a very rare occurrence, when the constitution has been once restored to its accustomed energy.

The divisibility † of matter is so incalculable, that it may be conceived possible for a species of irritation to prevail, of a similar kind to that which occasions the paroxysm of Asthma; in consequence

* Professor Storck's Works.

† I will acknowledge that I was much amused with your remark on this subject; when speaking of "the quantity of air extricated from "pounded apples," which, you observe, from Dr. Hales, "give out forty eight times their own bulk;" and immediately add, "this air must have been condensed into less than a forty eighth part of "the space it takes up when free from them"!!

Practical Enquiry, p. 267.

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of a stimulus, of which the particles are too minute, even when in their grossest state, to come within the sphere of our observation.

It is in these cases that the use of anti-spasmodic medicines is found to be of service: although the particular mode of their operation is very imperfectly understood.

It has been ingeniously suggested, that the power and utility of the resinous and fetid gums, consists in their action "as * conductors or retainers of something very similar to electric matter in the system:" but although I am induced to favour the same opinion, and could support it, by what I conceive to be, very cogent arguments, the consideration of this subject would necessarily extend my observations to too great a length: I shall, therefore, content myself, *at present*, with stating my opinion, that the purely convulsive Asthma is never found to arise unless the real cause of humoral Asthma prevail, in a greater or less degree.

I have now completed the task which I had proposed to myself,—namely,

I. To offer a few remarks on some passages in your "Practical Enquiry on Disordered Respiration."

* Baohé on Electricity.

II. To

II. To point out the existence of *acrid matter* in the *blood vessels*, in cases of Asthma.

III. To shew the manner in which the *serous effusion* into the pulmonary vesicles takes place.

IV. To explain the *nature* of the alteration which is produced in the *properties* of the blood.

V. To trace the offending matter to its *origin*, and to discover the *real cause* of Asthma.

VI. To suggest the principles on which its cure may be successfully undertaken.

I am aware that all Theory, though ever so well supported by the solid proofs of sound reasoning and accurate experiment, is liable to objections, which envious captiousness in some, and a laudable jealousy of innovation in others, are extremely ready to advance.

It may be the fate of my opinions to meet with such objections; but as the attainment of TRUTH is the great object of my views, I shall be happy to see the arguments which can be brought forward, in opposition to what I have advanced, *fairly* stated; and will endeavour to meet them with candour.

If they shall be found to subdue the force of what I am able to adduce, in support of these opinions,

opinions, I shall kiss the rod, with ready and respectful submission, and abandon, as it will become me, any errors which I may have propagated, to just correction: for if the improvement of medicine, and consequently the interests of society, can be advanced, by the subversion of my Theory, I shall relinquish it without regret, and be the first person to rejoice at the destruction of the fabric which I have erected; if its being razed to the ground, may only make room for a more perfect structure, though *not one* of the materials which I have collected, should be used in the building of it.

The importance of the subject which I have endeavoured to elucidate, will, I trust, recommend my remarks to the serious consideration of the faculty: and as you have excellently observed, that "good humour never fails to return with the freedom of respiration", I hope, at the same time when I congratulate you on your own cure, I may assure myself, that you will receive these pages with that generous candour which dignifies learning; and is the brightest ornament of science.

I remain, Sir,
with all due respect,
your most obedient,
and most humble servant,

GEORGE LIPSCOMB.